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DAMASCIUS

LECTURES ON THE PHILEBUS WRONGLY ATTRIBUTED TO OLYMPIODORUS TEXT, TRANSLATION, NOTES AND INDICES

by

L. G. WESTERINK

The anonymous commentaries on the Phaedo and the Philebus which were appended to the collected commentaries of Olympiodorus about 900, have long been an unknown quantity in Neoplatonic studies. Though they are commonly referred to as the work of Olympiodorus, they are evidently far superior to the standard that prevailed at Alexandria in the sixth century. Recent research has pointed towards Damascius as their main source.

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EXTERNAL SANDHI IN INDO-EUROPEAN

Summary

Contextual effects cannot be disregarded in the historical study of IE sounds, but it is unsafe to take this fact as an invitation to discount even word-boundaries as irrelevant, where convenient. Sanskrit above all has enticed historians into doing so. Apart from general considerations and the evidence derived from phonological study of other IE languages, recent attempts to use 'IE external sandhi' to solve particular problems are unconvincing.

Historical comparatism, like all investigating disciplines, suffers by the law of diminishing returns. To compensate, ingenuity looks about for fresh ways of re-aligning the data and increasing their power of suggestion — or perhaps has another look at old ideas with the same object. Much play as from time to time been made (for historians have never ignored 'structure') with the tendency of some IE languages to develop and organize, so to speak, those variations of word-sounds which arise from contextual position within the word or sentence or intermediate syntagm, and to regularize a series of differentiations with even an observable reflex in the orthography. Brugmann to be sure, under the general heading of *Satzphonetik*¹), deals with an assortment of phonetic (often deduced from graphic) deviations from the expected, among them that of anomaly at the beginning or end of a word under the influence of sentence-position, which is usually called 'external sandhi': and this is offered, as often since, as a significant factor in language-history.

Suspicion and doubt leap up at once. Do we speak of an IE feature? If so, although there is no objection to extrapolation from the phenomena of even a single particular IE language, it is essential to quote those phenomena correctly and at the same time not to infringe other accepted canons of 'IE behaviour', or not to do so, at least, unwillingly and unwittingly. But in IE, if anywhere, the word is 'institutionalized' as an operational element: no speakers within this large and varied field appear at all doubtful as to the size or shape or number of these

¹) *Grundriss* ² I §§ 1001—35, from which the relevant material has to be extracted.

elements in their discourse. Indeed, enclisis and proclisis, verbal reduplication, the loss of initial *p-* or *m-* — these are all, despite superficial appearances, indications (by way of being variations on a basic theme) of a strong consciousness of word-boundaries, especially of where words begin. Liaison, regular in French, sporadic in English (*not at all*) and Greek (Myc. *te-ko-to-na-pe*), has the same quite limited power to set up persistent anomalous forms as have oddities like wrong division; and metrical syntaxis (or accelerated speech), with elisions and crasis and the like, has no substantial generalizing effect (for *lendemain*, *impraesentiarum*, *προὔδος* etc. are individual stereotypes, which deserve in any case to be classed under the rubric of internal sandhi). Besides, there is on the one hand the predominant role of initial/medial/final position as a contextual factor in establishing IE sound laws, which makes all the more suspect an appeal to structure which overrides this feature of order; and on the other hand evidence is not lacking to suggest that the speakers of IE languages were (*a*) more aware of word-division than of morpheme-junction, and (*b*) — where the latter made some impression — more conscious of the boundaries of grammatical morphemes than of lexical ²). Clearly a word, especially as a dictionary item, was and is grasped as a stable and not easily divisible whole.

This is not entirely so in the language which is taken as the model for these supposed IE (eu)phonic variants, Sanskrit: hence the differing attitude of Indian and Greek grammarians to the word-sentence relation, (and the Greek readiness to treat inflection and derivation as one) ³). But the extrapolation is not rigorously made. Sanskrit obviously fails to conform to the requirements of those who try to fashion from its external sandhi a handy historical tool for comparative study, and this in two respects: they demand generalization of the syntagmatic (even alongside the supposed *in pausa*) form, but cannot point to **āpi* or **chrutvā* in Sanskrit as separate lexically recognized items ⁴); and they seek to transform an apparent initial or final word-

²) e.g. (*a*) μέλαν ἦπαρ, but φαίνω: (*b*) μέλιττα (partial integration), but *te-tu-ko-wo-a*, δεδορκώς. Cf. W. S. Allen, *Lingua* vii, 2 (1958), 119 and fn. 31. (The co-existence of ὑποδιαστολή and paragogic —v (cf. R. H. Robins, *TPS* 1957, 82) shows equal awareness of word and syntagm as entities).

³) Cf. R. H. Robins, *TPS* 1957, 91 and 72 fn. 3.

⁴) Cf. H. Galton, *Zeitschrift für Idg. Forschungen*, lxii, 2 (1952), esp. 170 and 173. The initial consonant mutations of Welsh are equally non-generalizing.

position of an intractable phenomenon into a medial one, on a presumption which in practice equates external with internal sandhi, whereas these are independent and even diverse processes in Sanskrit (e.g. *tañ veda* but *jaganvān*, *prabha ehi* but *gave*).

The only objection to using some sort of sandhi for historical purposes within individual languages arises if and when violence is done to the known phonological structure of the language under review ⁴⁾; the general doubts aired above do not apply. Moreover, it should not worry anybody if the appeal is to internal effects in this connexion (although this normally takes the form of particular and exceptionless laws of 'assimilation' and 'dissimilation' (or some equivalent prosodic statement) for common sound sequences. And these laws are so widespread a phenomenon (despite their not being identical from language to language) that internal sandhi might harmlessly be applied in quoting even IE 'words', if anybody feels optimistic enough to risk that nowadays ⁵⁾. A further step, equally reasonable, is to accept and even expect the same process inside sequences which for historical or purely conventional reasons are orthographically subdivided into 'words', but which are monolithic in terms of intonation or by the criterion of commutation (so, in the article-noun group, Cretan *τὸνς/τὸς* before following *V-/C-*, for privative variation, or Attic *τῆμ πόλιν* etc., for qualitative ⁶⁾).

External sandhi, on the other hand, is no such easy notion even in the historical exposition of single languages — those, that is, which do not show it as a thoroughgoing element of sentence structure. Here one looks for a clear and frequent source-syntagm, together with

⁵⁾ If the arguments of O. Szemerényi, *Proc. 7th. Intern. Cong. Ling.*, II 482, are accepted, our proto-IE will be monosyllabic and all internal sandhi ultimately to be viewed as external: but root-extension and polysyllabicity may still precede euphonic shifts.

⁶⁾ So Cyp. *ta-po-to-li-ne*. Cf. Lejeune, *Traité de Phonétique grecque* ², 274 fn. 2. Buck (*Greek Dialects*, 1955, 82) quotes cases like *ποιοῖεγ κατὰ...* — but these are rare and only affect final nasal. Some look for a variation in dative plural forms (as *-a/o ις* + *V/-a/o ισι* + *C*): but this is not traceable in poetic practice or dialect epigraphy. If it ever operated, it has already been subjected to homalizing in Mycenaean, if *-a-i/-o-i* are general dative forms and reflexes of *-αισι/-οισι*. IE *o*-stem instrumental/locative differentiation, with partial Greek assimilation, is the likeliest explanation and the Mycenaean contexts do not suggest otherwise.

either an understandable generalization or unmistakable doublets (or both). Many facultative mutations are quotable from languages where *Satzphonetik* is a matter of free variation ⁷⁾: but it is very hard, and may be impossible, to point to a stable form which is anomalous in terms of *prima facie* historical explanation and which can be arrived at *via* the generalizing of a contextual variant — at least if there is no relaxing of strictness of distinction between the two types of sandhi. To find actual doublets (and thus be enabled to ignore hypothetical influential syntagms, because of already having the diversified end-products) is easier ⁸⁾. But here, too, there are worries and uncertainties. To take Greek as the battlefield, it has invitingly been suggested that a structural alternation is to be seen in the vocalizing of certain zero grade ablaut forms involving semivowels (καρδία but -καρδίη): and to this the objections, if numerous, are precariously balanced ⁹⁾. For instance, even if the divergence is granted in the supposed pair δασύς/δαυλός, it is tempting to refer it to a spreading of purely internal effects ¹⁰⁾ (so perhaps δρακεῖν from ἔδρακον etc. or cf. ὑπόδρα) — but a glance at the paradigms will show that analogy and levelling have irretrievably obscured the issue: regularized ἐτάρπην etc. (τέρπω) opposes itself to

⁷⁾ See, for example, for Greek, Lejeune, *Traité* ², ch. x ('le mot dans la phrase'). It is ingenious to use (p. 275) the alternation -*ōi* > -*ō(y)-V/-ōi-C* in supposed external sandhi, and so explain 2.s. βούλη(ι)/βούλει etc. But the usual formulation of 'Osthoff's law' is faulty, depending as it does in part on fictitious IE long diphthongs; and suffix variation (e.g. -*eH/-ei*) will solve the early internal cases of this 'elision'/'shortening' (Zṯy-/Zéy — *gām/goṣu* etc.). βούλει, like κλείς, (and unlike τῆ(ι), affected by τᾶ(ι)) is no doubt referable to a closing process to be understood within the framework of a fourth century and later phonological realinement of Attic vowels and diphthongs. In Thessalian and Boeotian the early closing of long *e/o* vowels obscures the issue.

⁸⁾ δ(υ)ώδεκα seems acceptable, although the forms are not visible side by side except in Homer.

⁹⁾ H. M. Hoenigswald, *Language* xxix (1953), 288ff. Cf. the criticism in *Arch. Ling.* 8 (1956), 126 (but it must be admitted that πράσσον is a poor witness, and θλάω — cf. *dhṛṣad* — supports Hoenigswald). It is easy to believe a process **dnēsulos* > **dnVsulos* (V = vocalic phoneme) > **dnV-ulos*: hard to accept **dnV-ulos* > δα-υλος. Moreover ἄρα/ῥα and καρδ-/κραδ- do not behave alike in Homer.

¹⁰⁾ Δάειρα must be from **δασερία* (?*dnēs-*), and this from δέδαε (cf. δάω by analogy) or one would expect /*dnēs*-, with -σ- retained. Θάρσος maintains θράσος (retaining -σ-) by its parallel existence, and yet this existence itself is maintained by a (slight) lexical opposition: this is precariousness indeed.

ἐτράπην etc. (τρέπω) and so on (Hom. φιλότῃ τραπεῖομεν — from τέρπω despite θ 292: see Chantraine, *Gram. Hom.* I, 400 — is a 'temporary' metrically determined, choice); and this robs internal sandhi of any special influence. Or again, one might speak of dialectal differences (δαρχμά, δαρχμή and the like); loans would be easy and the common language has no objection, phonologically speaking, to the pattern /CrVC. But at a sufficiently early stage it may be possible to see the two forms in a single dialect (and that before any chance of inter-borrowing): so perhaps in Myc. *to-no* (utterance initial) against (*o-pi e-sa-re-we*) *to-ro-no-wo-ko*¹¹). Now if Kurylowicz is right in assigning to syllabic scripts a special tendency to morphological (and above all lexical) 'pull' on the orthography — if, for instance, the *scriptio plenior* of Hitt. *har-ap-pa-an-zi* owes its otiose second sign to the desire to double the visible lexical connexion with the singular *har-ap-zi* (the most economical writing of that form)¹²) — it may be that Myc. *we-ke* (-φέργης?) is differentiated from *we-re-ke* (φέρει) on this principle. Then the splitting of *to-no* from (lexically identical) *to-ro-no-* is all the more surprising and demands the adducing of another, more powerful principle; and external sandhi may be the best candidate (the syntagmatic position is right). Yet there remains a final doubt — apart from the admitted uncertainty of the Myc. transcriptions — namely that if external sandhi can override the claims of lexical indication it must be stronger than the internal variety, and to say this is to reverse their apparent relative power in Greek phonology as a whole. At best, one can only enter a cautious 'not proven' verdict on these sporadic effects, as they seem to be, of outer context¹³). To set them up as formulae applicable to all (or

¹¹) Myc. *to-pe-za* seems to give the doublet to τράπεζα, but the latter is curious, if *q^utr-ped-ia is its origin. ταρπ- would be expected in any case; and as early as Myc. one looks for *qe-to-ro-pe-za (cf. *qe-to-ro-po-pi*). No doubt the etymology is obscured, perhaps by (earlier?) τρίπεζα (Boeot. acc. to Hesychius, otherwise τρέπεδδα) — a more useful article on rough floors.

¹²) J. Kurylowicz, *Proc. 8th. Intern. Cong. Ling.*, 220f. (neat explanation also of the apparent Hitt. anomaly in the form of the zero grade nasal infix in verbs).

¹³) If the reflexes of IE semivowels, as seen in Vedic, suggest a division of the structure CSV- into (ncn-absolute initial) -CSV- (functioning like VCSV-) and (absolute initial) /CSV- (functioning like CCSV-) (so Edgerton, *Language* xix (1943), 83ff. passim), there is nothing to tie the former of these with specifi-

many of) the languages of a genetically related group is not very wise.

There would be less inclination to withhold belief in the syntagm as an *IE* factor in the histories of sounds and sound-complexes if two pieces of evidence were forthcoming: first, a notable proportion of alternating forms (or potentially alternating forms) in contextual positions where shift or omission or intrusion is likely or paralleled; and secondly, a liberal quotation of doublets both intralingual and interlingual. In fact, only one alternation measures up reasonably adequately to these qualifications — that of initial *sC-/C-*. Where pairs exist lexical distinction is commonly absent, which brings to mind a continued free variation involving a naturally unstable sound: but an original *IE* sandhi relation remains possible; unique, it may be, but still possible ¹⁴).

Other demonstrations of this so-called *IE* activity, however, must remain under something of a cloud. A recent theorist, accounting for the diverse first person singular verbal endings ¹⁵), uses internal (enclitic) and external sandhi together (in the sequence verbal root + pronominal suffix + following word); and the reflexes are as unobjectionable in the former case as they are unbelievable and unnecessary in the latter. From his enclitic element *H(m)* (cf. $*e\acute{g}e/oH$: $*e\acute{g}He/o\acute{m}$), itself preceding absolute final position, it is not difficult to deduce $-\bar{o}$ or $-\bar{h}(i)$ ($-V + H/$), $-\bar{o}m(i)$ ($-V + Hm$) $-\bar{e}/\bar{o}/\bar{a}m$ ($-H + Hm/$) ¹⁶) $-\bar{a}$ ($-C + He\acute{m}/$), $-\bar{o}m$ ($-C + He/o\acute{m}/$) — if one is prepared to accept such an origin for the 'thematic' vowel, with medio-passive $-ai$ as an

cally external composition in languages other than Vedic. Nor does the attempt to explain Gk. fem. ending $-\bar{a}$ via acc. $-\bar{i}\bar{a}\nu < -\bar{i}\bar{m}$ need external sandhi either: the pattern $atyu/$ gives acc. $-\bar{i}\bar{m}/ > -\bar{i}\bar{a}/$, which did not 'feel' right and received its final $-\nu$ through the same instinct as gave its $-\alpha$ to $Z\eta\nu\alpha$, and replaced the $-\alpha$ - of $\kappa\epsilon\lambda\tau\alpha\iota/o$ etc. by $-\nu$.

¹⁴) The sequence *VC* is the only one within which the emergence of *s* is not seen in the *IE* languages (for $CC > CsC$, as for the elisions $VsC > VC$, $CsC > CC$, see the grammars). Hoenigswald, bringing into line *SHV-/HV-*, avoids the lexical (near-) identity seen in the $(\sigma)\tau\acute{\epsilon}\gamma\omicron\varsigma$, $(s)melt$ type (an opposition is more likely to maintain both terms of a contextual alternation); but he specifically rejects *IE* sandhi as the source of the alternation, preferring sandhi in particular historical languages as the retaining influence (*Language*, xxviii (1952), 182ff.).

¹⁵) Heller, *Language*, xxxiii (1957), 19ff. The realizations of his syntagmata are hard to relate to known historical reflexes.

¹⁶) E.g. $*e-g^ueHa-$ (cf. $*g^ue-g^ueHa+Ha > \beta\acute{\epsilon}\beta\bar{\alpha}\chi\alpha?$) $+ Hm/ > *eg^ueHa\acute{m} > \acute{\epsilon}\beta\bar{\alpha}\nu$.

extension to this form, and $-\alpha\mu\tilde{\alpha}v$ perhaps showing conflation with the $-H_2 + Hm/$ reflex. This is at least as successful as the bringing in of the following word as an attempt to sort out this particular complex.

Another essay in the use of this awkward device sets more problems than it solves. Martinet has recently extended Sapir's notion that $-H + H + V- > -kV-$, to account for the initial consonant of Sl. *kostъ*, Lat. *costa* (but Hitt. *ḫastai*, Gk. *ὀστέον*, $< *H_3est-$)¹⁷⁾, and has afforded a good example of the general and particular drawbacks inherent in this approach. In general, it is unsound to meddle with word-initial position in IE as a whole, and no better to treat a random section of the IE field so; and external sandhi in Slavonic is more than suspicious¹⁸⁾. Moreover in this particular case (even if we admit *os/costa* as a doublet, and ignore theories by which *kostъ* derives its initial from a prefix ($*p)kw-$)¹⁹⁾, it is odd that the supposed context (fem. adj. $-aH + H$ - fem. noun) does not produce Lat. $*canus$ (*ḡanas*) — or, if H_3 is demanded by the hypothesis, $*cavis$ (for H_3 here, see Benveniste, *Origines*, 176); even more, while the internal sandhi which is the model ($-H + H- > -k-$) is probably acceptable in Greek, ($\xi\sigma\tau\rho\omega\chi\alpha < *e-streH_a^{20}) + Ha^{21}$), Latin appears specifically to reject it ($*bhleH_a^{20} + Hai^{21} > flāvī$). True, one may be tempted to say that the lip-rounding feature makes all the difference ($-H^uH- > -HH^u- > -H^u-$), and that $\xi\sigma\tau\rho\omega\chi\alpha$ is 'analogically' affected by forms like $\xi\theta\eta\chi\alpha$ ($*e-dheH_1 + Ha$) — or, to make it sound better, that $-H^uH-$ in Greek 'falls together with' $-HH-$. But then, if $-H^uH-$ 'falls together with' $-H^u-$ in proto-Italic, as one expects²²⁾, one still looks for the necessary

17) *Proc. 8th. Intern. Cong. Ling.*, 51.

18) Cf. Galton, *op.cit.*, passim.

19) So P. Thieme, *Die Heimat der idg. Gemeinsprache* (1954), 45f., binds *kostъ* to Sl. *koza*, Lat. *caper*. Hazardous as this use of the 'pecus' prefix may be, it is as safe as deriving non-fem. *caper* from $-H$ *Haper*; or are we to work from *capra* $< -H$ preceding ill-attested (H)*apra*?

20) For $eH_a^{20} > \tilde{a}^u C/\tilde{a}uV$ see A. Martinet, *Word* 9 (1953), 257.

21) $-Ha$ is a reasonable quotation for this ending, although it argues ostensibly for e.g. $\pi\acute{\epsilon}\rho\omicron\mu\psi\alpha < *πεπομπ + Ha$, whereas the Homeric evidence shows the $*te-trp-ntai > τετράφαται$ type as the start-point for the 'aspirated' perfect. For $-Hai$ (or $-Haei$ or some American equivalent — so Hamp, *Word* 11 (1955), 402f.) see now Kuryłowicz, *Proc. 8th. Intern. Cong. Ling.*, 236, adducing Anatolian (Luwian etc.) $-ha +$ presentive $-i$ (with the corroboration that $-ta-i$ becomes $-ti$, not, as primary $-ti$, $-zi$).

22) Hamp. *op.cit.*, 403.

Latin internal shift *-HH-* > *-k-* (e.g. *-H-Hai* > *-cī*). But the forms are lacking: *fēcī* and *iēcī* (the so-called 'strong aorists') are to be divided *fēc-ī*, *iēc-ī* (cf. *fac-io*, *iāc-io*), the dorsal being now a root element and the words no doubt early loans accepted in a metanalysed, *durchgehend* form — cf. Cypr. δώκ-οι: they are therefore irrelevant, and nothing else offers itself.

It follows that a sound statement of the criteria which will allow us to admit the influence of the whole utterance upon the part in working out the histories of stable, repeated and separately quotable IE word-sounds — that this statement is a prime necessity and will help enormously. Until it appears, external sandhi is an attractive but expensive and unreliable toy for historical phonologists, or — worse still — a mechanism for the *ad libitum* disregarding of the provisions of sound-laws. After all, Leskien gained a notable if not always remembered victory in 1876; let us not too lightly surrender the ground then won.

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N. E. COLLINGE

A NOTE ON STATISTICAL METHODS IN COMPARATIVE LINGUISTICS

Summary

It is desirable to distinguish between *lexicostatistics* as a means of proving relationships between languages or linguistic groups not previously known to be related, and *glottochronology* as a means of measuring the time depths of separations between languages or linguistic groups already known to be related.

For proof of relationships *structure statistics*, i.e. the application of statistical methods to comparisons of structural elements, needs to be stressed as an indispensable complement of lexicostatistics.

This is illustrated with tests.

There are, in the literature on lexicostatistics, or glottochronology as it is also called, two things which have struck me as remarkable and worthy of comment. The first is that nowhere, as far as I can see, a clear distinction is made between what I would call glottochronology in the stricter sense of the word, i.e. a means of dating separations of languages or linguistic groups known to be related, and lexicostatistics as a means of proving genetic relationships between languages or groups not previously known to be related. This fact is, perhaps, best illustrated by the indiscriminate use of the two terms for the same technique. And yet "lexicostatistics" is a more general and neutral expression than "glottochronology" which implies an historical element that is lacking in "lexicostatistics". This merging of two distinct concepts may be partly due to Swadesh's own simultaneous treatment of them, although to the attentive reader the difference should be perceptible enough¹). Also the subject has been treated largely by anthropological linguists who are often more interested in the time depth possibilities it offers than in its linguistic importance as such.

Apart from purely logical and methodological considerations – the time depth of a relationship can only be estimated after the re-

¹) Compare for instance pp. 312 sqq. with p. 326 of Swadesh's "Perspectives and problems of Amerindian comparative linguistics" (*Word*, 10, 1954), where "the problem of proof" and "the problem of determining the order of relationship" are treated respectively.

lationship itself has been established – there is this in favour of making the distinction, that in large areas, such as New Guinea for instance, there is as yet no question of measuring the time depths of linguistic splits, but only of patient and prudent attempts at reducing the complex pattern of extreme linguistic diversity. And to this end lexicostatistics as a means of establishing and proving relationships can be of importance, while glottochronology as a means of measuring the time depths of the relationships is, linguistically, premature, as long as we have not even an outline picture of the latter.

There is, however, a second and more important argument in favour of making a distinction between the two concepts and, consequently, between the two terms. This is that many of the objections that can be raised against lexicostatistics are concerned with the assumption – a “discovery” according to Swadesh ²⁾ – that the basic vocabulary of a language tends to be replaced at an approximately constant rate, and that this rate is uniform for all languages. This assumption has not been proved. It was found correct in a test of only thirteen languages covering a history of little more than 2,000 years ³⁾, and Gudschinsky ⁴⁾ rightly doubts that the reality of the assumption has been adequately demonstrated since Swadesh himself ⁵⁾ has for various reasons discarded as invalid ten out of the thirteen test languages, although in the same article he continues to speak of the rate as uniform. I have myself ⁶⁾ criticized Swadesh’s “discovery”, adducing examples where the theory would not work (nota-

²⁾ “Lexicostatistic dating of prehistoric ethnic contacts” (*Proceedings of the American philosophical society*, 96: 452–463, 1952). Cf. also Swadesh, “Archeological and linguistic chronology of Indo-European groups” (*Am. Anthropol.*, 55: 349 sqq., 1953), and Swadesh, “Mosan I: A problem of remote common origin” (*IJAL*, 19: 26–44, 1953).

³⁾ See also R. Lees, “The basis of glottochronology” (*Language*, 29: 113–127, 1953).

⁴⁾ “Three disturbing questions concerning lexicostatistics” (*IJAL*, 22: 212–3, 1956a). Cf. also her “The ABC’s of lexicostatistics (glottochronology)” (*Word*, 12: 175–206, 1956b).

⁵⁾ “Towards greater accuracy in lexicostatistic dating” (*IJAL*, 21: 121–137, 1955).

⁶⁾ “Prospects of a ‘Papuan’ comparative linguistics” (*Bijdragen Kon. Inst.*, 113: 70–91, 1957).

bly as between Dutch and English and between Dutch and German). To this I can now add a comparison between Old Netherlandic (Old Low Frankish) of the so-called Carolingian Psalmfragments of Wachendonck, dating from the beginning of the 10th century A.D. 7), with New Netherlandic (Dutch). This shows that in ca. 1.05 millennium out of 68 words in the old language, occurring in Swadesh's 100 word test list, fully 63 words, or 92,6%, were retained, or 61 words, or 89,7%, if the words for "many" which is *manage* in ON, and for "hair" which is **lok* in ON (gen. s. *lockis*, acc.p. *locka*) are not counted as retentions because NN *menige* and NN *lok*, though common words, are in every day language usually replaced resp. by *veel*, *vele* (= Eng. "much" and "many": ON *uilo*, *filo*- is rather = Eng. "much" than "many"), and by *haar* (NN *lok* is rather = Eng. "lock of hair"). The other "losses" are: ON *mikil*, NN *groot*, "big"; ON *luttika* (nom.p.), NN *klein*, "small" (although NN *luttel* still exists but in restricted and somewhat modified usage); ON *quethan*, NN *zeggen*, "say" (although ON *farseggon* occurs once in the sense of "excuse"); ON *thu*, NN *jij*, "thou"; and ON **reslân* (ger. *te reslâne*, 2 p.p. ind.praes. *reslât*), NN *doden*, "kill" (NN *slaan* = "strike, beat"; *verslaan* = "beat, defeat, slay"). This result, which means a retention rate of 93% or with a very strict count 90,2%, per millennium, is not only very different from the retention rate of approximately 81% per 1,000 years used for Swadesh's old 215 word test list, but also substantially higher than the rate of approximately 86% used for the 100 word list (Swadesh 1955: 127). It lies between the maximum rates recorded for German (90%) and Swedish (94,4%) which latter, according to Swadesh, there is reason to believe has been scored too high by about 5 percentage points (*ibid.*: 130-1). This result is the more significant since the ON Psalmfragments are in an eastern Low Frankish dialect, while modern Dutch is mostly based on a western Low Frankish dialect. This may perhaps account for such a "loss" as e.g. ON *thu*, since NN *jij* is due to Frisian or Ingweonic (i.e. western) substratum influences. Also, since the text is so fragmentary, chance may have played a role in that the simple verb

7) See for this text recently H. K. J. Cowan, "De oudnederlandse (oudnederfrankische) psalmenfragmenten" (*Textus Minores*, ed. E. J. Brill, Leiden, 1957), and the literature mentioned there.

**seggon* (cf. *farseggon* mentioned above) is not recorded as against *quethan* which is; the closely related Old Saxon, at any rate, has *seggian* as a synonym of *quethan*. Similarly OS has *grôt* (NN *groot*) as a synonym of *mikil*. The retention score for the 68 words used could, therefore, actually prove to be even higher if the material were richer than it is. Also I doubt that the percentage would be appreciably lower if the full test list of 100 words were available instead of only 68 words. In fact, out of an additional 13 words occurring in the older 215 word list only one "loss" was counted: "at", which is expressed by *an* in the ON text and by *te* in NN, *to*, *te* in ON being rather equivalent to Eng. "to". This addition, therefore, does not lower the retention rate. The least one can say is that there are reasons for grave doubt about the constancy and uniformity of the ratio for all languages.

It is not necessary, of course, to accept the validity of the theory in order to draw conclusions concerning *relative degrees* of relationship as soon as the relationships are proved and the percentages of cognates in the compared languages known. These percentages themselves supply the necessary data for that. What is impossible or not proved to be possible is the fixing of the degrees by *dating* the times of separation of the proto-languages. Speaking of "an *indicated* divergence time, representing both the duration and degree of separation" and which is "not necessarily an actual divergence time" (Swadesh 1954: 326), does not help much since the *time* measurement remains with its dubious elements.

Now the assumption of a constant and uniform retention rate is not, as is sometimes said (e.g. Gudschinsky 1956a: 212), the basis of *lexicostatistical* theory, but of *glottochronology* in the sense that I have just attributed to this term. The basis of *lexicostatistics* is the probability calculus. And against the validity or the usefulness of this calculus itself for linguistic purposes no serious doubts, as far as I can see, have been raised. Why should they? There is no reason why the calculus should not be as useful to provide a reliable measure of proof in linguistics as it has been for other disciplines, provided that it is translated in a form appropriate to that end. What is subject to discussion, therefore, are the *requirements* that should be postulated to make the probability calculus applicable for linguistic purposes. This being so, we can, if necessary or so desired, easily raise the

minimum standards for accepting proof. We can, with Fairbanks ⁸⁾ raise the phonetic expectancy ratio, and/or, with Kroeber ⁹⁾ use longer test lists (although Swadesh, 1955, in spite of Kroeber, *reduced* his original list in order to discard items that had proved to be either non-universal or not sufficiently non-cultural etc.), and/or, with Fairbanks, Gudschinsky (1956b) and W. Bright ¹⁰⁾, use stricter rules for judging word agreements if phonological correspondences are not previously known. But none of these criticisms seriously questions the lexicostatistic technique as such.

More serious seems to be Hoijer's critique ¹¹⁾, according to which "none of the items of the test list are necessarily 'relatively neutral in their cultural implications', to say nothing of their being 'universal and non-cultural'", and which judges it doubtful that any test list, though "taken as a whole (it) may well represent an area of experience common to all mankind, (can) also provide a division of this area into one hundred 'easily identifiable broad concepts which can be matched with simple terms in most languages'". Yet Hoijer himself made almost unqualified use of the technique for his later "Chronology of the Athapaskan languages". His criticism is, without doubt, too severe. He admits himself that "it is true, of course, that we can avoid items which are obviously culture-bound (like most kinship terms, or terms related to a particular political system or set of religious beliefs) and that items can be excluded which have reference to geographic and climatic factors of restricted distribution". Only he adds that "what cannot be avoided is the particular patterning of categories and types that a language and the culture to which it belongs impose upon lexicon and vocabulary. . . ." I cannot see, however, – and Hoijer, in applying the method to Athapaskan, apparently does not think so himself – that this should invalidate any test list, let alone the entire technique. In its extreme consequence it would mean that *no* two comparative lists are ever

⁸⁾ "A note on glottochronology" (*IJAL*, 21: 116–20, 1955).

⁹⁾ "Linguistic time depth results so far and their meaning" (*IJAL*, 21: 91–104, 1955), especially p. 97.

¹⁰⁾ "Glottochronologic counts of Hokaltec material" (*Language*, 32: 42–48, 1956).

¹¹⁾ "Lexicostatistics: a critique" (*Language*, 32: 49–60, 1956). Cf. also Hoijer, "Chronology of the Athapaskan languages" (*IJAL*, 22: 219, 1956).

valid, whatever their composition and however large the number of agreements, and this would unsettle the basis for *all* comparative linguistics. But the final paragraph of his critique seems to indicate that it is the errors by overestimating vocabulary losses that he fears and the resultant errors in retention percentages and times of separation (i.e. errors in *glottochronology*) rather than errors in judgment of relationships.

Nevertheless, Hoijer's objections are important enough to question the advisability of using *lexical* statistics alone. And examples from more than one area increase this doubt. Thus in Malay at least 7 of the 97 "non-cultural" words of the test list used by Swadesh in his second experiment (1954) appeared to be Sanskrit loanwords, thus yielding a percentage of more than 7 of apparent cognates (Cowan, 1957). In the Achehnese language of North Sumatra there were 22% cognates with the Mon-Khmer languages of Indo-China as against 67% cognates with Austronesian or, where this language does not conform with Austronesian as reconstructed by O. Dempwolff, with Malay. The residue of ca. 10% could not be placed, possibly because of insufficient data in Mon-Khmer. For the close relative of Achehnese, the Cham language of Indo-China, the figures were not much different. In the case of these two languages the situation was further complicated because of the distant relationship that undoubtedly exists between the Austronesian and Mon-Khmer families as such, a fact that is also of importance for structural and morphological comparisons (Cowan, *ibid.*). In New Guinea *mainland* Melanesian languages little more than 35% of the vocabulary appeared to be Austronesian, although the distribution of the AUN material is not the same for every single language. This means that a considerable quantity of lexical material must belong to a Papuan substratum. This conclusion finds support in the fact that the Papuan substratum that Capell¹²) reconstructed in the Melanesian languages of South Eastern Papua includes no less than 37% of the words occurring in the 97 word list mentioned above, although these do not all occur in every single language (Cowan, *ibid.*). On the other hand non-AUN ("Papuan") languages sometimes show a considerable number of loanwords from AUN, which in e.g. Lawuna of S. E. Papua amounts to 14% of the

¹²) "The linguistic position of south eastern Papua", Sydney, 1943.

test list. A further complication is that sometimes even certain structural features of morphology and syntax are borrowed from AUN by Papuan languages or found in AUN (Melanesian) as Papuan substratum features (Cowan, *ibid.*).

As I pointed out for the Achehnese-Cham case, "the answer to the problem cannot be satisfactorily given by applying Swadesh's methods alone. An approach along broader lines is needed" (*ibid.*, p. 75). "However, as ... Swadesh's methods ... present a workable basis for the lexical comparison of languages that have diverged widely, they deserve(d) to be tried in the Papuan field" (p. 77). For "quite apart from the question whether comparison of grammatical structures alone will ever be sufficient to prove genetic relationships, there is the simple fact that for many languages only vocabularies and no grammars are as yet available" (p. 71). Recently Swadesh himself ¹³⁾ expressed similar ideas, saying that "in establishing a genetic relationship, either structure or lexicon may be used as the first clue, but both must be involved in the final proof".

This takes me to the second point: the application of statistical methods for *structural* comparisons. It is, at this point, only fair to recall that some early applications of statistics in linguistics were precisely in the fields of phonology, morphology and syntax ¹⁴⁾. But the tendency seems to have been towards re-adapting the techniques used there to lexical ends. Swadesh (1954: 313) has, in a footnote, expressed credit for Björn Collinder's first effort at a mathematical proof of relatedness, adding that this approach was based rather on structural elements than on vocabulary, as if this deserved less attention, although it is true that Swadesh did briefly point to such recurring elements in Amerindian as *n* for the first person pronoun and *m* for the second (*ibid.* 311-2). Kroeber and Chrétien ¹⁵⁾ improved Jan Czekanowski's "method of differential diagnosis by quantitative correlation determinations" for the classification of IE languages, resting "upon the recognition of isolable and definable features or

¹³⁾ "Problems of long-range comparison in Penutian" (*Language*, 32: 22, 1956).

¹⁴⁾ See the literature mentioned by Bj. Collinder, "La parenté linguistique et le calcul des probabilités" (*Uppsala universitets årsskrift*, 1948), p. 1.

¹⁵⁾ "Quantitative classification of Indo-European languages" (*Language*, 13: 83-103, 1937).

traits". But Chrétien recently ¹⁶⁾ used the technique for determining word distributions in the Melanesian languages of South Eastern Papua.

For *structure statistics*, as it could be called in contrast with lexicostatistics, both comparisons of material elements of morphology and of non-material traits or features of principle may be tried. Both were used by Jan Czekanowski ¹⁷⁾ in his quantitative classification experiment in IE. Of the latter he included such items as conjunctive, optative, dual in verbs, dual in nouns; of the former augment, suffixes like *-bhis* and *-mis*, verb-ending in *r* etc. Kroeber and Chrétien (1937) on the other hand did not include non-material features in their list. But in these experiments the object was classification of a group of languages known to be related and with a long, well-known history. To that end non-material features of the kind mentioned may well be of use. But for the comparison of languages not known to be related but whose relationship one tries to establish and prove, they are too abstract. They are not isolable and definable like morphological elements, and some categories, even when they seem similar, are not readily comparable from language to language because a different interpretation may be given to them as between different languages. In an experiment with four widely separated Papuan languages the present writer attempted, along similar lines, though somewhat simplified, as Collinder's but necessarily with the use of non-material features only, to establish a connection between these languages, and to prove that this connection cannot be due to chance, thus establishing a large area of apparently related languages. The languages were chosen (1) for their dislocation as extreme West – extreme East and extreme mid-North – extreme mid-South, and (2) for the fact that they belong to the very few which have been studied sufficiently though not ideally. They were, therefore, sufficiently random. The results seemed satisfactory enough in that the languages in question had in common a substantially higher number of features than the chance expectancy out of a random sample of 50. But on more than one point doubts rose about the interpretation of categories as between

¹⁶⁾ "Word distribution in southeastern Papua" (*Language*, 32: 88–108, 1956). Chrétien used Capell's material, cf. note (12) above.

¹⁷⁾ "Na marginesie recenzji P. K. Moszyńskiego o Książce: Wstęp do historii Słowian" (*Lud*, Ser. II, Vol. II, 1928), cited from Kroeber and Chrétien, 1937.

the compared languages. Also it is uncertain, and will probably always be uncertain in such cases, whether the alternative of chance here must be necessarily genetic relatedness. Many of the features which characterize Papuan – for although “Papuan” is little more than a cover term for those languages in and around New Guinea that are in a negative sense *not* Austronesian they *have* such traits in common – are also found in other linguistic groups elsewhere in the world, notably in those which are sometimes with an unhappy name called “primitive”. This fact calls for an explanation, but not more so than the fact that certain features seem to be common to *all* or most languages in the world. What did come out fairly clearly was the closer unity of “type” of the Papuan languages *as against Austronesian*, which latter was for contrast included in the experiment. But this fact is nothing new and is precisely why they have been given a separate name for themselves, though not a genetic group name.

The other approach, use of specific morphological elements, is much more concrete. As mentioned *en passant* it was Collinder, in his comparison of Uralic and Altaic (1948), who first employed structural material for a proof of relatedness. He used nominal case-endings and conjugation affixes, though also pronominal stems. This first effort of Collinder’s has been criticized by V. Pisani in the present journal (Vol. III, 1952–3, pp. 1 ff.). But the main objections, such as the question as to whether the alternative of chance is “parenté, emprunt” or “affinité élémentaire” ^{17a}), were mostly eliminated or considerably reduced in the later experiments of Swadesh and others. And it is precisely in order to further test and improve the method that experiments have been and should be continued. Apart from the somewhat elaborate method used by him, the importance of the statistical comparison of structural matter as an indispensable complement of lexicostatistics deserves to be stressed. That this approach can be very useful and in certain cases even be the only expedient becomes clear when we re-examine my comparison by Swadesh’s 1954 method of the West Vogelkop languages in Western Netherlands New Guinea with the non-Austronesian languages of North Halmahera

^{17a}) I leave aside here the question of the true relation between “parenté” and “emprunt” as put by Pisani in this connection, because it does not especially concern these statistical methods but in fact the *entire* problem of the “genetic” relationship of languages.

outside New Guinea. One of the Vogelkop languages in question, Ajamaru (or Mejbrat, as it is also called), yielded "only one dubious CVC comparison ... and two CV correspondences (with N. Halmahera) ... This makes no case. Relationship can be reconstructed by chain procedure ... though by a very round about way The chain relationship which can be constructed, however, finds support in the strongly suggestive and significant fact that the pronouns, including those that do not occur in Swadesh's 97 word list, show a close likeness to those in the NH languages ... notably also ... the pronominal prefixes in use in conjugation". This was shown in a table, and the conclusion added that "the chances that all this is coincidence are practically negligible" (Cowan 1957).

If we now test these pronominal subject prefixes by Swadesh's recently proposed more precise new method (1956: 38-9) we obtain the following results. We arrange the material in a table, this time using the nearly extinct Ibu dialect of North Halmahera only, because this has the more archaic forms as van der Veen ¹⁸) has shown. The prefixes are mostly CV but, apart from Ibu 3 p.s.m. *o-* and 3 p.s. non-hum. *i-*, only the consonants are considered because the vowels are unstable in Ajamaru, dropping before stem vowel and sometimes also before consonant, and because their phonological relations cannot be determined.

<i>person</i>	<i>Ibu</i>	<i>Ajamaru</i>
1 s.	to-	t(e)-
2 s.	no-	n(e)-
3 s.m.	o-	j(e)-
3 s.f.	mo-	m(e)-
3 s.n.h.	i-	—
1 p.ex.	mi-	—
1 p.in.	ne-	n(o)-
2 p.	ni-	n(e)-, b(o)-
3 p.	na-	n(e)-, m(e)-

In some NH dialects the 3 p.s.m. is *wo-*, the 1 p.p.in. *wo-*, *po-*, *fo-*,

¹⁸) "De noordhalmaherese taalgroep tegenover de austronesiese talen" (doct. thesis, Leiden, 1915), pp. 180, 184, 187 and 188-9. For Ajamaru see J. E. Elmberg, "Notes on the Mejbrat people of the Ajamaroe district" (Stockholm, 1954, mimeographed), and Cowan, 1957.

ho-, and the 3 p.p. *jo-*, *ǵo-*, *du-*. For non-human things in the plural the singular is used in NH and it can, therefore, be omitted from the comparison. In Ajamaru the 2 and 3 p.p. each have two different forms in different conjugations, which will be included in the calculation in order to observe strict objectivity. In Ajamaru no separate exclusive form of the 1 p.p. exists. In this language also there is no special form for the 3 p.s. non-hum., the 3 p.s.f. being used to that end. Apparently the distinction here is between male humans and everything else as against the NH distinction between humans, subdivided in males and females, and everything else. Since, therefore, class contents appear to have shifted a little, it is only fair to count Ibu 3 p.s.n.h. *i-* and Ajamaru 3 p.s.m. *j(e)-* as corresponding because the shift in class content may very well have been accompanied by a shift in forms. This is, moreover, largely confirmed by the fact that the incorporated *object* affix for the 3 p.s.m. actually is *-i-*, *-wi-* in NH. Also the 3 p.p. form *jo-*, *ǵo-* of other NH dialects seems to be related.

We have, then, $9 \times 9 = 81$ total pairs involved in the comparison, with 22 possible agreements if meaning is not considered, viz. I. *to* = A. *t(e)*; I. *no*, *ne*, *ni*, *na* each = A. $3 \times n(e)$ and $1 \times n(o)$; I. *mo* and *mi* each = A. $2 \times m(e)$; I. *i* = A. *j(e)*. This makes a chance expectancy ratio of $\frac{22}{81} = 27,16\%$. The actual number of correspondences if meaning is taken into account is, however, 7 out of 9, or 77,77%; that is more than $2\frac{4}{5}$ times what could be expected from chance. Even without admitting I. *i* = A. *j(e)* the actual agreement percentage is 66,66%, that is nearly $2\frac{1}{2}$ times the chance expectancy.

The chance expectancy ratio, as calculated according to Swadesh's new method, was checked empirically with a simple experimental device, namely the use of two sets of 9 "Scrabble" game letters each, corresponding to the two sets of single prefix phonemes involved in the comparison under discussion. The two sets were set apart in two heaps with their backs up, and, after shuffling, two letters, one from each heap, were taken simultaneously and at random, and turned. The number of times that an identical pair ("agreement") showed on turning, was noted. This experiment was repeated a number of times planned in advance, viz. 50, so that $50 \times 9 = 450$ pairs of letters have been turned. The result was 114 agreements, or 25,33% of 450; the average was, therefore, $\frac{114}{50} = 2,28$ agreements per experiment, or 25,33% of 9. There is, then, only little discrepancy between the

calculated and the empirically determined chance expectancy ratios; if they differ, the former does so on the safe side rather than otherwise. In this connection it is, perhaps, worth noting that after the first ten experiments the chance ratio was 23,33%; after twenty: 27,77%; after thirty again 27,77%; and after forty: 25,55%. The 114 agreements in 50 experiments consisted of 2×5 , 6×4 , 8×3 , 22×2 , and 12×1 . The maximum of potential chance agreement (obtained only twice) was, therefore, 5 out of 9, or 55,55%. This is still substantially less than our actual agreement percentage of 77,77, or 66,66 if I. *i* = A. *j*(*e*) is not admitted.

Against the argument as set out above possibly two methodological doubts may be raised. One is whether in calculating the chance expectancy ratio meaning is really not taken into account, considering that it is *personal* elements we consciously choose to compare. And the other is whether the list of elements is a sufficiently large and objectively chosen sample. As to the first objection I would point out, however, that *any* two test lists, whatever their contents, are necessarily made up for the meanings of their items. What is meant, of course, is the import of meaning when *comparing* the two lists. The second objection seems stronger, but it must be remembered that lexical items consist of phoneme complexes, while the present material consists of single phonemes only. The objectivity requirement was taken into account as well as possible by including in the calculation the 2 and 3 p.p. prefixes of different conjugations in Ajamaru.

However, for confirmation we now shall also test the material with a more elaborate method similar to that of Collinder's. As Vendryes¹⁹) has said, "le nombre des phonèmes possibles s'étend presque à l'infini Mais il s'en faut beaucoup que les langues utilisent à la fois toutes les ressources du langage. Le nombre des phonèmes de chaque langue est au contraire assez limité". Bloomfield²⁰) says that "the number of simple primary phonemes in different languages runs from about fifteen to about fifty". The average per language can therefore, be put at ca. thirty. Collinder put the total of possible phonemes hypothetically at 300 and the average per language at 50, but in these he includes complex phonemes. For

¹⁹) "Le langage" (1950), p. 40.

²⁰) "Language" (1950), p. 90.

simple primary phonemes we may safely put the number at a total of 100; this prevents a too favourable presentation. Our average of 30 per language is well in keeping with the number in NH (28, see Cowan 1957: 87), while Ajamaru, which does not distinguish between long and short vowels and between voiced and unvoiced plosives, has less (Elmberg 1954: 1, 10).

The number of known linguistic families in the world is estimated at ca. 150, but some 4/5 of these are small Amerindian groups which are, moreover, in a permanent course of reduction. We can, therefore, safely take into account only a number of 30, the more so considering the isolation ever since neolithic times of Papuan by Austronesian to the West, North and East, and by Australian to the South.

These figures are tabulated in a table consisting of 30 horizontal lines (for the 30 linguistic families) and 30 vertical columns (for the average number of phonemes per language), thus forming 900 squares. A pack of 100 numbered cards (for the total of possible phonemes) is prepared, the pack is shuffled and 30 cards of it are turned one after the other for the 30 squares in the first horizontal line, in each of which the corresponding number of the card is noted. This procedure is then repeated 29 times (for the other 29 horizontal lines), each time, remixing the 30 cards with the 70 remaining cards of the pack, and taking good care that the complete pack is reshuffled very well every time. What is the probability that two lines at least will show a minimum of 4 correspondences? For safety we count only 4 correspondences instead of 7, viz. I. *to* = A. *t(e)*; I. *i* = A. *j(e)*; I. *n* = A. *n* only once; and I. *m* = A. *m* only once, because the other 3 correspondences do not contain different phonemes. The results were as follows: in 72 cases one agreement, in 15 cases two agreements, and in 3 cases 3 agreements as between any two lines. Not once 4 agreements occurred, and the maximum of 3 was so rare that we may safely state that this experiment confirms the findings of the other test.

These results make a very good basis for assuming that the agreements between Ibu and Ajamaru are not due to chance; and if not to chance, to what else than common origin, since there can hardly be any suspicion of borrowing here, nor of sound-imitation or cultural binding. Also, no objections like those against non-material features can be raised here. And in any case the results are a positive confir-

mation of the chain relationship that could be arrived at in a very round-about way by applying lexical statistics. This case shows that sometimes a connection can be demonstrated by structural elements even when the vocabulary does not give the slightest suggestion of relationship. Incidentally, a case like this makes an attempt at glottochronological dating illusory, for what is the use of a vocabulary retention rate where no vocabulary retention is apparent?

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PLANNED AND UNPLANNED DEVELOPMENT IN MODERN HEBREW *

Summary

After many centuries of semi-fossilized existence, Hebrew has regained its normality. The development of the re-created language, Modern Hebrew, is in certain respects a phenomenon which is of interest to the general linguist. Aspects of planned versus unplanned development in the domains of phonology, morphology, and vocabulary are here described, in an attempt to clarify the relationship between the new constituents of Modern Hebrew, the products of recent processes of evolution, and the old constituents, which are legacies of the past. Of the above three domains, phonology appears to be the least responsive to linguistic planning. In the field of vocabulary, on the other hand, the planned activity has been quite successful. The products of the processes of morphological development would perhaps be best described as spontaneous results of the need for suitable means of expression; to a certain extent, however, they have been influenced by the activities, planned as well as unplanned, in the vocabulary domain.

For many generations primarily a literary and liturgical language, today Hebrew is a full-fledged language in Israel – a recognized means of communication in a modern society. Two-thirds of a century ago, there arose the idea to make Hebrew, the ancient national language, once more the common tongue of the Jews of Palestine. At that time, the spoken languages of the Jewish population of Palestine were mainly Yiddish in the Ashkenazi communities, and Judeo-Spanish and Arabic in the Sephardi and Oriental communities. As this idea was carried from vision into reality, a new era in the history of the language opened: a process of transformation began by which Hebrew, becoming a spoken tongue again, regained its normality.

The revival of Hebrew, closely tied up with the Zionist movement, was part of the general renaissance of Hebrew culture in Ottoman and Mandatory Palestine (as now it is in Israel), and, at the same time, an essential means for the accomplishment of such a renaissance. In the Hebrew linguistic revival, we observe the two typical features

*) The original version of this paper was presented as a contribution to the Conference on Economic Planning and Social Policy in Israel, Harvard University Center for Middle Eastern Studies, May 10 and 11, 1958.

of every renaissance, which may be called eclectic continuation and innovation ¹⁾. The continuation is *eclectic* because only certain aspects of the past are retained; others are rejected. Those aspects of the past upon which the renaissance has been based are complemented by innovations.

It is possible to trace the development of Modern Hebrew in terms of the processes of eclectic continuation and innovation. Needless to say, it is only upon historical considerations that this distinction between the two categories of processes can be based. From a descriptive point of view, there is neither need nor justification for such a distinction. I have found it necessary, however, to point this double-featured character of the transformation, since it is of relevance for the subject of this paper. One may consider the question of the planned and unplanned development in Modern Hebrew to be centered around two relationships. The first is the relationship between the old constituents, which are a legacy of the past, and the new constituents, which are products of recent development. The second is the relationship between the conscious and subconscious processes of transformation.

Let us now describe in some detail the planned and unplanned development of Modern Hebrew. We shall study this development in the domains of phonology, morphology, and vocabulary ²⁾.

I

In the early stages of the transformation, the question of the *correct* pronunciation – i.e., the pronunciation which should be adopted by

¹⁾ In the application of these concepts to the revival of Hebrew, the writer follows Professor N. Rothenstreich's analysis of the aspects of cultural renaissance (in a lecture given at the Harvard Hillel House on November 10, 1957).

²⁾ My thanks are due to Dr. Ben Halpern and to Professors Charles A. Ferguson, Morris Halle, and Chaim Rabin for their remarks regarding various points dealt with in this paper.

A note on transliteration and transcription: Hebrew grammatical terms, names of books and articles, as well as all other words which do not serve as examples of Modern Hebrew will be *transliterated*; on the other hand, words cited as examples of spoken Hebrew will be *transcribed*, their transcription being based on the pronunciation current in the "General" variety of Israeli Hebrew (see below).

speakers of the language – was a highly controversial matter. In fact, the debate between proponents of the Ashkenazi and the Sephardi pronunciations began much earlier than the movement for reviving Hebrew. In 1808, Moshe Lemans published a book entitled *'imrā šerūphā* ("Pure Speech") which sought to prove that the Sephardi pronunciation of Hebrew was the correct one. This book was followed by a series of publications by various authors advocating either the Ashkenazi or the Sephardi pronunciation ³). However, it was not until the last quarter of the nineteenth century, when spoken Hebrew became a reality, that the discussion of pronunciation ceased to be merely a matter of learned argument.

In order to understand the nature of the discussion about the problem of "correct" pronunciation, we have to bear in mind the specific history of Hebrew. For a period of nearly 1700 years, Hebrew existed orally not as a spoken language but as a liturgical language, the language in which prayer was conducted and sacred texts read and studied. At the time when the revival of spoken Hebrew began, the various Jewish communities differed, as they still do today, with regard to their traditional Hebrew pronunciations, that is the pronunciations which were preserved in the reading of sacred texts. Roughly speaking, these pronunciations may be divided into three major groups: the Ashkenazi [main features: distinction between *qāmēš* and *pathaḥ*, as well as between *šērī* and *sēghōl*; realization of the *qāmēš* as [o] or [u]; no regular realization of the *šewā mobile*; realization of the hard t as [t] and of the soft t as [s]; no pharyngeals and no emphatics; no gemination; stress usually non-ultimate], the Sephardi [main features: pronunciation of *qāmēš* as *pathaḥ*, i.e., as an [a], and of *šērī* as *sēghōl*, i.w., as an [e] ⁴); realization of the *šewā mobile* as a short [e] ⁵); realization of the hard t as [t], and the soft t either as [θ] or as [t]; preservation of the pharyngeals and of some emphatics; ⁶)

³) Cf. A. R. Malachi, Ben Y^ehūdā w^ehammibḥā hass^efāraddī ("Ben Yehuda and the Sephardi Pronunciation"), *Bitzaron* 19 (5718 [1957–58]), 15–20.

⁴) Usually, a mean-mid or lower-mid vowel; some Sephardi pronunciations, however, have a higher-mid vowel as the realization of the *šērī* = *sēghōl*.

⁵) Or as a mean-mid central vowel [ə].

⁶) The pharyngeals and the emphatics have been preserved in most varieties of the Sephardi pronunciation – primarily in Arabic-speaking communities, but not in all of them. For further details, see I. Garbell in *Bulletin de la Société de Linguistique de Paris* 50 (1954), 233 ff.

gemination; stress either ultimate or penultimate], and the Yemenite [main features: distinction between *qāmēš* [ɔ] and *pathaḥ* as well as between *šērī* and *seghōl*; no distinction between *pathaḥ* and *seghōl*, which are both pronounced [æ]; realization of the *ḥōlam* as [ö], or, in some regions of Yemen, as the *šērī* ([e]); realization of the *šewā mobile* as a short [a]; differentiation between hard and soft *bgd kpt*, as well as between pharyngeals and non-pharyngeals, emphatics and non-emphatics; gemination; stress usually non-ultimate]. It should be noted that each of these pronunciations, and particularly the Ashkenazi and Sephardi, exists in several varieties.

The problem that the leaders of the linguistic revival faced at the first stages of the transformation was: What pronunciation should be made the standard pronunciation of Hebrew? The Yemenite pronunciation, being employed by only one community (and, besides, hardly known in Palestine and Europe at that time), could not have been taken into account; the decision, therefore, had to be made between the Ashkenazi and Sephardi pronunciations.

The decision was made in favor of the Sephardi pronunciation, on which Modern Spoken Hebrew (= MSH) is fundamentally based. The reasons for this decision, which was strongly backed by the vehement activity of Eliezer Ben-Yehuda (1858–1922), “the Reviver of the Hebrew Language”, were manifold: In certain communities which employed the Sephardi pronunciation, Hebrew was occasionally used as a conversational language even prior to the first stages of transformation, and it was believed, therefore, that this pronunciation would serve as a better and more “natural” standardized foundation for the revival. Some circles considered the Sephardi pronunciation to be more “aesthetic” or more correct from a historical point of view than the Ashkenazi. The Sephardi pronunciation symbolized, to its adherents, the spirit of the cultural renaissance, whereas the Ashkenazi, highly reminiscent of Yiddish, represented the Diaspora, the direct continuation of the immediate past which they rejected.

Having accepted the Sephardi pronunciation as its established standard pronunciation, MSH began to develop in the phonological domain, as in the other domains, along lines of its own. When we compare the actual phonological system of Israeli Hebrew ⁷⁾ with the

⁷⁾ The term Israeli Hebrew (= IH) is used here to denote the contemporary

phonological system of Hebrew in the traditional Sephardi pronunciation, we observe that the dominant variety of Israeli Hebrew – the “General” Israeli variety – does not possess quite a few features of the Sephardi pronunciation. Another variety of Israeli Hebrew, the “Oriental” variety, shows some Sephardi features which are not possessed by “General” Israeli Hebrew⁸). Both “General” Israeli Hebrew and “Oriental” Israeli Hebrew, however, have lost certain features of the Sephardi pronunciation. In the following table, the main phonetic features of the traditional Sephardi pronunciation are compared with those of the above two varieties of IH.

The phonemes	Traditional Sephardi pronunciation	“General” Israeli Hebrew	“Oriental” Israeli Hebrew
<i>I. Vowels</i>			
historical <i>qāmēṣ</i> historical <i>hōlām</i> historical <i>šēvī</i>	[a], as <i>pathah</i> [o] [e], as <i>sēghōl</i> , both when the <i>šēvī</i> is followed in the orthography by a <i>yōdh</i> and when it is not followed by a <i>yōdh</i>	[a], as <i>pathah</i> [o] [e], as <i>sēghōl</i> , but usually only when the <i>šēvī</i> is not followed by a <i>yōdh</i> ; a <i>šēvī</i> followed in the orthography by a <i>yōdh</i> is usually pronounced [e]	[a], as <i>pathah</i> [o] [e], as <i>sēghōl</i> , usually both when the <i>šēvī</i> is followed by a <i>yōdh</i> and when it is not followed by a <i>yōdh</i> ^{8a}).
the <i>šewā</i>	the regular parallel, according to traditional rules, is a short [e] or an [ə]	no regular parallel; existing parallels vary between zero and [e], and their occurrence is to be explained by the phonemic rules of Israeli Hebrew	as in “General” Israeli Hebrew (in most sub-varieties of “Oriental” IH)
<i>II. Consonants</i>			
the pharyngeals / ʕ / and / ħ /	exist (in most varieties)	replaced by / ' / (or zero) and by / x /, respectively	exist
/ w / / r /	[w] ⁹ lingual [r]	[v] mostly velar [ɣ]	[v] lingual [r]

spoken Hebrew of Israel; MSH refers to modern spoken Hebrew from its inception.

⁸) For these varieties of IH see Haim Blanc in *The Middle East Journal* 11 (1957), 399–400.

^{8a}) Some sub-varieties of “Oriental” IH, however, have [ei] for a *šēvī* which is followed by a *yōdh*.

⁹) In some varieties of the Sephardi pronunciation only; others have [v] as the realization of historical / w /.

The phonemes	Traditional Sephardi pronunciation	"General" Israeli Hebrew	"Oriental" Israeli Hebrew
4. the emphatics:			
a) / ʃ /	pronounced emphatically: [ʃ]	pronounced as the affricate [ts]	pronounced as the affricate [ts]
b) / ʈ /	pronounced emphatically: [ʈ]	pronounced [t]	pronounced [t]
c) / q /	pronounced emphatically: [q] ¹⁰⁾	pronounced [k]	pronounced [k]
5. soft t	pronounced [θ] or [t]	pronounced [t]	pronounced [t]
<i>Consonantal allophones</i>			
gemination	extant according to historical rules	nonextant (in its traditional sense)	as in "General" Israeli Hebrew (in most sub-varieties of "Oriental" IH)

III. Prosodic features

1. stress	ultimate in certain morphological categories; penultimate in others	in most cases ultimate or penultimate in accordance with the traditional Sephardi pronunciation; penultimate, however, in certain categories in which the traditional Sephardi stress is ultimate. In some cases, the stress is antepenultimate or preantepenultimate	usually as in "General" Israeli Hebrew
2. intonation	no intonation extant ¹¹⁾	intonation patterns of its own	intonation patterns of its own

We may say that the pronunciation of MSH is Sephardi, or, to be more exact, non-Ashkenazi, only in four of its main features: in having [a] as the equivalent of historical *qāmēš* and [o] as that of historical *ḥōlam*; in having [t] – and not [s] – as the equivalent of the historical soft t; and in being based upon the Sephardi stress system. These features are in general agreement with the speech habits of speakers

¹⁰⁾ Some of the varieties of the Sephardi pronunciation which have not preserved the emphatics (see above, note 6) have [ts], [t] and [k] as the respective realizations of historical / ʃ /, / ʈ / and / q /. In a small number of communities (Aleppo; some of the North African communities), / q / is realized as [ʔ].

¹¹⁾ The traditional Sephardi pronunciation is primarily, as are all Hebrew traditional pronunciations, the pronunciation of a *liturgical* form of Hebrew. In its liturgical form, Hebrew is recited according to various traditional melodies; we have, therefore, to do here with musical *melodies* but not with the linguistic feature of *intonation*.

whose native tongues were Central or East European (or, at least, they do not clash with these speech habits). This is a result of a process of selection and rejection. In reviving Hebrew on the basis of the traditional Sephardi pronunciation, the speakers who participated in the early and middle stages of the transformation rejected (most, of course, subconsciously) those features of Sephardi pronunciation which were strange to the phonemic systems of their native tongues. Most of these speakers were members of Ashkenazi communities, and, as their native tongues did not possess (to take only two examples) either the pharyngeal consonants or the phonetic feature of gemination, these traits of the Sephardi pronunciation were not included in the phonemic system of "General" IH.

The rejection of the above features also gave MSH a distinct "Western", or European, character. Some non-Western features exist in "Oriental" IH, which in certain of its traits, is closer to the traditional Sephardi pronunciation than is "General" IH.

To sum up: In the phonological domain, the revival of Hebrew was based on a certain linguistic legacy, on the Sephardi traditional pronunciation. In the course of its development, however, Modern Spoken Hebrew has acquired a phonemic system which is only in part identical with that of Hebrew in the traditional Sephardi pronunciation. There was an act of planning in the early stages of the Revival – the decision to adopt the Sephardi pronunciation as a model, thus making the pronunciation of MSH distinctly non-Ashkenazi. In its development, MSH did not stick to this model; following the general rules of linguistic evolution, it was strongly influenced by the linguistic backgrounds of its speakers. This influence, as might be expected, is evident in many phenomena besides the aforementioned rejection of the pharyngeals and gemination. Among these phenomena we may list the introduction of new phonemes – such as / f /, / v /, the development of initial consonant clusters ¹²), and the voicing of consonants followed by voiced stops or by / z / ¹³), as well as the unvoicing of some

¹²) Classical Hebrew does not possess initial consonant clusters, nor do they exist, as a rule, in the traditional Sephardi and Yemenite pronunciations. For initial consonant clusters in IH, see Haiim Rosén's *hā'ibhrith šellānū* ("Our Hebrew"), 158 ff. (Tel-Aviv, 5716 [1955/6]).

¹³) E.g., [xežbón] "bill", "invoice", "arithmetic" (historical *xešbōn*); [aγzavá] "disappointment" (historical *'akhzāvā*). The positions in which the voicing in

consonants when followed by unvoiced consonants. All these phenomena, however, are to be explained both in the light of the foreign linguistic backgrounds of the Ashkenazi speakers and as resulting from processes of internal development.

From time to time, there have been attempts to make the pronunciation of MSH more "Semitic" or "Oriental" – that is, to restore to the speech inventory of its speakers some of the original features of the traditional Sephardi pronunciation which have been lost in the course of the development of MSH (such as the pharyngeal consonants, the lingual [r], and the gemination), and, on the other hand, to eliminate from the spoken language certain features which it has acquired (such as the initial clusters). These attempts were initiated by scholars and teachers¹⁴) who, in their speech, exemplified the phonetically desirable form of the language and urged its adoption. An organized body, the Council for the Cultivation of Speech, founded in 1946 by Professor S. D. Goitein under the auspices of the Hebrew Language Committee (known since 1953 as the Hebrew Language Academy), organized lectures and courses intended to promote a pronunciation of Hebrew which should be more "Semitic" as well as more aesthetic¹⁵). The success of these activities has been meagre.

Another attempt to form a standardized Hebrew pronunciation which would possess some of the "Semitic" traits has been made in the Hebrew programmes of the Palestine (since 1948: Israel) Broadcasting Service. Some of the announcers pronounce the pharyngeal consonants in their "Semitic" form, and all of them have a lingual [r] (the [r] of the "General" IH is velar). This tendency is, however, weaker now than in the first period of Hebrew broadcasting some twenty-five years ago. As far as we can now judge, it appears that the pronunciation of the announcers has not had any considerable effect on the speech habits of most of the listeners.

question occurs in IH are partially equal to the positions in which the parallel phenomenon takes place in Russian, and one may assume the existence of a Russian substrate to the IH phenomenon under consideration.

¹⁴) Mention should be made here of the names of the late Professor David Yellin and Dr. Y. Epstein.

¹⁵) See S. D. Goitein, *hammōcēšā leiharbūth haddibbūr mahūthāh ufeulātāh* ("The Council for the Cultivation of Speech, its Nature and Work"), *Lēšonenu* 16 (5708–5709 [1947–8/1948–9]), 55–65.

II

In the early stages of the transformation, there was no real need for planning in the field of morphology. The Revival was strongly backed by the literary activity of the writers of the second half of the nineteenth century and the early twentieth century. Of particular importance was the work of S. J. Abramowitz (better known under the pen name of Mendele Mōkhēr Sefārīm), who created a new Hebrew style – as a matter of fact, a new written language – which in many respects was an appropriate literary foundation for the revived spoken language¹⁶).

The leaders of the Revival either took part in the above literary activity, or, at least, were well-versed in its products. And, needless to say, they were acquainted with the classical sources of Hebrew. These sources could, of course, supply the morphological rules which were needed for the revived language and in them, as well as in the literature of the second half of the 19th century, the revived language found a sound morphological basis. As Hebrew post-Biblical texts differ from Biblical texts in many morphological (and morphosyntactical) aspects, the problem then could have been theoretically a problem of choice: What morphological rules should be adopted, the Biblical or the post-Biblical? This problem did not arise, however; in the literature of that period (and especially in the works of Mendele Mōkēr Sefārīm), a language had already been created, the morphological rules of which were, from a historical point of view, a result of blending Biblical usage with post-Biblical usage (this term being used here in its broadest sense). The language of this literature rejected certain elements of the morphological and the morphosyntactical legacy (such as the jussive and the cohortative, or the tense system of Biblical Hebrew), which were found to be redundant or felt to be unfitting; at the same time, other elements have been strengthened. This literary language guided the early morphological development of MSH.

In the course of time, and largely due to the necessity to translate European terms and concepts, the need arose for morphological innovations. Most of these “innovations” – to be more precise, we have to do here not with linguistic phenomena which are absolutely new to

¹⁶) See E. Y. Kutscher, Modern Hebrew and “Israeli” Hebrew, *Conservative Judaism*, Spring 1956, 30 ff.

the history of Hebrew but rather with an expansion of already existing principles – can be classified under the four following categories:

a) The derivation of new verbs from extant nouns. This was done mainly by employing the verbal formation CiCéC with roots possessing not more than three consonants (e.g. *mikkém* “[he] located” from *makóm* “place”) or the formations CiCCéC or CiCCCéC with roots, most of which were derived from European nouns, which have four and five consonants, respectively (e.g. *kitlég* “[he] catalogued” from *katalóg* „catalogue”; *tilgréf* “[he] telegraphed” from *télegraf* “telegraph”). Most Hebrew roots have three phonemes; this process, however, introduced into the language a large number of four and five consonant roots.

b) The formation of new nouns through a process of composition. This category includes several types of composition, which vary in the nature of the components. These components may be, to give just a few examples, two nouns (e.g. *kadurégel* “football” from *kadúr* “ball” and *régel* “foot”) or a noun and a newly-coined morpheme (e.g. *kolnó’a* “movie theatre” from *kól* “voice” and “*nó’a*” a morpheme from the root *n-w-ʔ* “to move”; *zrakór* “projector” from *zrak*, a newly-coined morpheme from the root *z-r-k* „to throw” and *ór* “light”). In certain cases, the products of the process of composition do not reflect the original forms of the components, but because of the effects of analogy and nominal patterning, reflect somewhat changed forms (e.g. *bin’úm* “internationalization”, originally derived from *beyn-leumí* “international”). In other cases, the process of composition originated in the written abbreviated form of a group of words. E.g. for *róš hamaté haklalí* “chief of general staff”, the abbreviation *rmtkl* was first used in writing; pronounced *ramatkál*, this form was soon accepted by the living usage.

c) The use of prefixes for the formation of new nouns and adjectives. These prefixes are originally Hebrew (such as *i-* “un-”, “dis-”), Aramaic (such as *tat-* “sub-”; *btár-* or *batár-* “post-”), or European (such as *pro-* “pro-”).

d) The use of suffixes, mostly borrowed from European languages, for the formation of new nouns and adjectives. Some of these suffixes are usually used with nouns which are loanwords by origin (e.g. *-íst* in *idealist* „idealist”; cf. *ideál* “ideal”); others come with Hebrew nouns (e.g. *-nik* “belonging to” in *kibútsnik* “a member of a *Kibúts*”).

The latter suffixes are mostly found in a substandard type of IH, which has a distinct colloquial colour.

By and large, all these categories of morphological development are the spontaneous results of a need for suitable means of expression¹⁷. To a certain extent, the process of morphological development has been influenced by the organized, as well as by the nonorganized, activity in the domain of the vocabulary (see below); on the whole, however, we have to do here with an unplanned process of internal linguistic adjustment to the needs of a modern society of a western type. To a considerable degree, the patterns of adjustment reflect contact with the structural principles of European languages.

There is another aspect of the morphology of MSH which falls within the scope of our discussion. I refer here to the problem of morphophonemic and morphological "errors". As a result of certain processes, which from a linguistic point of view are absolutely normal, the IH forms of certain nouns, adjectives, and verbs are different from their historically correct forms. To quote only one example: "sincere" is *kené* in IH, whereas the historically correct form of this adjective is *kén*. The form *kené* reflects both the analogy to the masculine singular form of other nouns, the feminine and plural forms of which are of the patterns CeCá and CeCím, respectively (*kehé: kehá: kehím* "dark" = *kené: kená: kením* "sincere") and the need to differentiate it from *kén* "yes". The same kind of "error" is reflected, e.g., in certain verbal forms. These processes, the results of which deviate, as stated above, from the norms of classical Hebrew, are in a limited measure counteracted by various activities, which attempt to eradicate the "errors" under consideration and to replace them by the forms which are correct from a historical point of view. The most important roles in these activities are played by the schools (and various "correct language" courses), the language sections of the daily newspapers, and the broadcasting service (both through its "language corner" and the exemplary usage of the announcers). The effects of these activities on IH usage cannot be estimated from data available.

¹⁷) For morphological developments in MSH see also Blanc, *op. cit.*, 401-407.

III

Very acute at the early stages of the Revival, the problem of the vocabulary has by now lost its prominence. Today, Modern Hebrew, both in its spoken and written forms, possesses a sufficient number of words to function normally. Some of these words, especially in certain technical domains, are, it is true, foreign, and there is still need for authorized terminological dictionaries in some fields – that is, dictionaries giving the authorized Hebrew equivalents of European technical terms. And, of course, even when an authorized term is officially proposed, it frequently has to struggle for acceptance by the Israeli speakers; in quite a few cases, the speaker would stick to a non-Hebrew term, rather than replace it by its Hebrew equivalent.

On the whole, however, the vocabulary problem of the revived language seems to have been solved, and this achievement should by no means be underestimated. We shall try to see here how Modern Hebrew overcame this problem. Let us recall that, at the earliest stages of the transformation, Modern Hebrew vocabulary lacked such everyday words as the equivalents of *kitchen* or *stamp*, not to speak of more abstract domains in which equivalents for such concepts as *institute* or *complex* were not extant. The *practical* need for words was then great, the *psycho-sociological* need perhaps even greater: the Revival had to prove, both to its opponents and to its followers, that it was indeed within the capacity of Hebrew to function as a spoken language again, and that suitable words could be produced.

The activities which contributed to the solution of the vocabulary problem can be classified under two headings – organized (or official) and nonorganized (individual) activities. Under the first category come the activities of the official language institutes of the Jewish community of Palestine and later on, of the State of Israel: the Language Committee (*Wa'adh Hallāšōn*, established in 1890 by Eliezer ben Yehuda) and the Academy of the Hebrew Language (which, in 1953, succeeded the Language Committee). Mention should be made here also of the Israel Defence Army's Committee for Military Terms, which, as its name indicates, deals with the special terminological needs of the Army. The second category includes the innovations created by various individuals. Some of these individuals are well

known as writers (notably Bialik and Shlonsky) or as statesmen (David Remez and Moshe Sharett); others are anonymous.

The individual activities were sporadic and spontaneous; on the other hand, those of the official language institute have been, and are still, mostly carried out according to a planned program. Apart from orthography, vocabulary is the only domain in the history of Modern Hebrew in which projects have been planned and carried out with a considerable measure of success. The reasons for this are clear: by the activities in the domain of vocabulary, an answer was given to an acute need; they constituted an intrinsic part of the transformation of Hebrew from a literary and liturgical to a spoken language; and, as there was no attempt here to impose upon the speakers any norms, rules, or regulations which were in conflict with their speech habits, no strong resistance was encountered. In other words: the planned activity in the domain of vocabulary did not constitute an intervention in the linguistic processes which take place in a language in development; it supplied some of the tools which the speakers needed, but, by the very nature of its subject, never attempted to dictate to the speakers any rules regarding the phonological, morphological, or syntactical use of these tools. Here lies the basic difference between the domains of phonology and morphology on the one hand and that of vocabulary on the other.

What lines have been followed in the planned, organized work in the field of Hebrew vocabulary? An answer to this question is given in the work program of the Language Committee¹⁸). According to this program, the planned development of the Hebrew vocabulary should proceed on two lines: the Hebrew literature of all periods and in all fields should be studied as a prime source for the needed words; words not supplied by these Hebrew sources should be formed by the Committee "according to the rules of grammar and on the basis of analogy"¹⁹). The above policies have been followed by the Language Committee throughout its history (1890–1953), and by its successor, the Hebrew Language Academy, which continues the work in this field by preparing and publishing various terminological dictionaries. There is, however, one significant point in which the planned activity

¹⁸) *Zikkhrōnōth wa'adāh hallāšōn* (*Memoires of the Language Committee*) 4 (1914), 77.

¹⁹) *Ibid.*, section III, b.

had to deviate from the original principles laid down by the Language Committee. I refer here to the adoption of non-Hebrew words. In its above-mentioned program of work, the Language Committee stated that "non-Semitic words, even such as are accepted in all Indo-European languages" ²⁰⁾ are not to be introduced into the usage. Taking into consideration the socio-cultural setting of the revival process, this attitude was too extreme to be realistic. In the course of its development, Modern Hebrew has borrowed a considerable number of words from European languages. (We are not concerned here with the borrowings from Arabic, these being in full accordance with the principles of the Language Committee.) Quite a few of these words are "international" (e.g. *psixológia* "psychology"; *univérsta* "university"; *rádío* "radio"); others are technical terms which may exist, particularly in the spoken language, side by side with their Semitic equivalents (e.g. the non-Hebrew *táyér* and the Hebrew *tsamíg* both denoting English "tyre"). In certain cases, the European loan-words in Modern Hebrew have not preserved their original meanings in full. *Enérgia* is the equivalent of English "energy" only when used as a concept in physics, other aspects of "energy" being conveyed by the Hebrew word *mérets*. A chemical *reaction*, as well as a personal reaction, have found a Hebrew equivalent in *tguvá*; as a historical concept, however, Hebrew uses *reáktsia* as the parallel of English *reaction* ²¹⁾.

It appears that the older Modern Hebrew becomes, the more tolerant is its attitude to non-Hebrew words. It is easy to understand why, at the early stages of the Revival, the official linguistic attitude towards borrowing from European languages was utterly negative. At those stages, when Spoken Hebrew had to struggle for existence, it was imperative to find *Hebrew* tools of speech for the needs of everyday life. A large scale non-Semitic borrowing could not have been accepted then as a solution of the vocabulary problem; creating a *langue mixte*, it would have undermined the whole process of the Revival. Later on, as a vocabulary largely composed of Hebrew items was gradually being established, borrowing began to look less perilous. Words which several decades earlier had an inferior standing are considered

²⁰⁾ *Ibid.*, section II, b, 3.

²¹⁾ See I. Avineri, *kibbūšēy hācibhrūth bedōrēnū* ("The Achievements of Modern Hebrew"), 62 ff (Merḥavya, Israel, 1946).

today to be fully legitimate. It may be of interest to note in passing that this change of attitude to the use of European words has its parallel in the change which occurred in the attitude of Hebrew speakers to the use of languages other than Hebrew by members of their community. Hostile and aggressive until the end of the thirties, this attitude is rather tolerant today: the use of other languages does not appear as a threat to Hebrew.

In reviewing the development of the vocabulary of Modern Hebrew, it would be appropriate to consider here a different kind of planning which has been taking place in recent years in Israel. Since the establishment of the State of Israel, scores of language courses have been established, which aim at acquainting the new immigrants with Hebrew in as short a period as possible. These courses ("Ulpānīm"), which range from a full-day teaching schedule over a period of several months to evening classes a couple of times a week, attempt to equip the students with a basic Hebrew vocabulary, as well as with the necessary morphological and syntactical notions, so as to enable them to communicate in Hebrew, both orally and in writing. To a considerable extent, these courses have been efficient tools in accomplishing this goal. This holds particularly for the "concentrated" type courses in which the language is taught at least for a number of hours a day over a period of several months. Needless to say, no direct act of planning the development of Modern Hebrew is involved in these or in other projects, such as the broadcast of news in "Easy Hebrew", which come to facilitate the linguistic integration of the new immigrants. However, by their very essence, the projects under consideration play an important role in shaping the vocabulary, as well as the syntax, of a not to be neglected part of the Israeli society; a certain influence is hereby exerted upon the form of Hebrew employed by a considerable number of speakers. It is too early to determine to what extent this influence proves to be of a permanent nature.

* * *

The leaders of the movement which brought about the Revival of Hebrew were successful in achieving their aim. Today, two-thirds of a century after the beginning of the transformation which made it a spoken language again, Hebrew is the national tongue of the State of Israel, and its vitality and ability to serve both as a means for every-

day communication and a vehicle of literary creativity and scientific expression are doubted no longer. In the social and political life of the Jewish community of Palestine and the State of Israel, Modern Hebrew has played a major role, the importance of which cannot be exaggerated.

Needless to say, the face of Hebrew today is not what was imagined by its revivers. This is normal, as every student of language knows, in the course of such developments. The very essence of language imposes considerable limitations upon the possibility of planning or of any deliberate intervention in the linguistic domain. This applies mainly to phonology, morphology, and syntax ²²⁾; vocabulary and orthography ²³⁾ reveal a greater degree of responsiveness to planning and intervention. From the linguistic point of view, there is, therefore, nothing extraordinary in the fact that, of the domains that have been reviewed in this paper, the phonology and the morphology of Modern Hebrew have lent themselves to planning in a lesser degree than the vocabulary. This will seem all the more normal if we consider a fundamental difference between the various categories of linguistic data which were available to the revivers at the early stages of the transformation. In phonology and morphology, the basic data existed in the Hebrew linguistic legacy. For the first domain, they were supplied by the traditional pronunciations which were preserved in the reading of sacred texts; for the second, by the usage of the classical literature. In these domains the problem was eclectic: Which of the existing pronunciations should be adopted? Which morphological principles and elements should be discarded? In regard to vocabulary, on the other hand, the revivers faced a problem of *vacuum*: the speakers needed many Hebrew words which were not extant. Planning – that is, laying the foundations of an organized activity – was essen-

²²⁾ It is practically impossible to review the syntactical development of Modern Hebrew, in both its written and spoken forms, as necessary preparatory studies of sufficient scope are lacking.

²³⁾ A detailed discussion of the problem of orthography would exceed the scope of this paper. It may, however, be of interest to note here that the spelling reform of the Language Committee has been effective to a fairly large degree. This reform ("the 'full' orthography") is based upon two principles: on the one hand, it seeks to introduce into the orthography hitherto missing vowels; on the other hand, it attempts to differentiate between consonants and vowels which heretofore were represented in the orthography by the same sign.

tial here. In fact, the needs in the domain of vocabulary were to a great extent covered by the planned activity of the official bodies (see above); it was not, however, possible – and one may wonder whether this would have been desirable – that the planned activity of the official bodies would be the only source to supply the speakers with their much needed tools. There was room for non-official activity, based upon the linguistic sense of certain speakers and guided by their intuition. Neither the official nor nonofficial activity could, however, exclude a process of borrowing from other languages.

Were the fathers of the Revival able to listen to the “General Israeli” Hebrew of today, their major disappointment would be so it seems, in the domains of phonology and syntax. Of the three domains of Modern Hebrew that have been reviewed in this paper, phonology appears to be the least responsive to linguistic planning. In 1914, the Language Committee, laying down the guiding principles of its work, set up two aims: the first was to make Hebrew capable of serving as a spoken language in all branches of life; the second, “to preserve the *oriental* [my emphasis] character of the language and its peculiar and characteristic form in the pronunciation of letters, in the construction of words and in style; to give it the flexibility necessary in order to express in full the contemporary thought of mankind”²⁴). However, the Language Committee, which was instrumental in making Hebrew a spoken language, could not impose upon the pronunciation of Modern Spoken Hebrew a pattern of oriental characteristics. How could there be successful planning in the phonological domain, if in any linguistic domain at all, in a country the population of which “has been formed by waves of immigrants hailing from all parts of the world. The intensity of immigratory streams has been such, that, at certain periods, the majority of the population and the overwhelming majority of the adult population were foreign born”²⁵). In the light of these circumstances, the story of the Hebrew Revival sounds closer to fantasy than to reality. It is indeed unique.

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²⁴) *Zikhrōnōth wa'ad h hallāšōn, ibid.*

²⁵) R. Bachi, A Statistical Analysis of the Revival of Hebrew in Israel, *Scripta Hierosolymitana* 3 (1955), 179.

DIE AUSDRUCKSFORM FÜR „VERÄUSSERLICHEN“ UND „UNVERÄUSSERLICHEN BESITZ“ IM FRÜHGRIECHISCHEN (Das Funktionsfeld von homer. φίλος)

Zusammenfassung

Das homerische Griechisch besitzt eine formelle Opposition „veräußerliches“/ „unveräußerliches Besitzverhältnis“, ausgedrückt durch a. adnominales φίλος gegenüber adnominalem ὁ oder αὐτοῦ (ohne Possessivpronomen), b. adnominalen Dativ gegenüber adnominalem Genitiv des Personalpronomens (letzteres ergibt sich bei Überprüfung des Verhaltens des Havers'schen „Dativus sympatheticus“). Eine formell mögliche genaue Abgrenzung der „Inalienablen“-kategorie des Homerischen läßt den Vergleich mit den von C. C. Uhlenbeck, Lévy-Bruhl und anderen abgegrenzten Parallelkategorien anderer (nicht-indogermanischer) Sprachsysteme zu. – Die syntaktischen Verbindungsmöglichkeiten von φίλος bei Homer (wie z.B. auch im Lesbischen) sind eng begrenzt und denen eines Pronomens praktisch identisch. Vom Vokativ abgesehen, steht es 1. attributiv nur bei Bezeichnungen von inalienablen Gegenständen, 2. prädikativ nur mit bezugsbezeichnender Dativergänzung und nicht in Bezug auf Inalienabilia, 3. substantiviert (nie mit beschreibender Adnominalergänzung) a. nichtprädikativ im masc. pl., b. prädikativ, c. im n. pl. mit Dativergänzung (wie 2); ferner steht es 4. in vokativischer Anrede. Jeder der genannten syntaktischen Stellungen läßt sich ein modernes Übersetzungsäquivalent zuweisen (1. „eigen“, 2. „zueigen“, 3.a. „Angehörige“, b. „was (jemandem) zu eigen ist“, c. = 2, 4. „mein“, substantiviert „Monsieur etc.“); das Funktionsfeld von homer. φίλος ist also nicht lexikal-semantic komplex, sondern syntaktisch gegliedert. Nachhomerisch fallen die syntaktischen Einschränkungen weg, φίλος wird nichtpronominales Adjektiv und volles Substantiv, und aus (2) entwickelt sich „lieb“, „Geliebter“ in allen Stellungen.

I

1. Die aus den gebräuchlichen Darstellungen des homerischen Griechischen geläufige Feststellung, daß φίλος entweder „lieb“ oder „eigen“ heißt (wobei die etymologisch-semasiologische Priorität der einen der beiden Bedeutungen oft unentschieden bleibt), erweckt den Eindruck einer semantischen Aufspaltung des Begriffsfeldes von φίλος oder den eines typisch homerisch-griechischen Begriffsfeldes, das etwa der Summe der deutschen Begriffsfelder „lieb“ und „eigen“ kongruent wäre. Eine nähere Überprüfung der Gebrauchsweise von

φίλος in den beiden großen homerischen Epen führt jedoch zum Schlusse, daß die den beiden deutschen Übersetzungsäquivalenten „lieb“ und „eigen“ entsprechenden Gebrauchsweisen eher syntaktisch auseinandergehalten sind, sodaß es für das Homerische gar nicht zutreffend ist, von einem Begriffsfeld „lieb + eigen“ zu sprechen; es wird sich vielmehr erweisen, daß gewisse syntaktische Konstruktionen von φίλος ein Übersetzungsäquivalent „eigen, angehörend“, andere wieder, von den ersteren deutlich geschiedene, etwa ein Übersetzungsäquivalent „nahestehend“, eventuell (jedoch selten) „lieb“ ermöglichen. Der Funktionsunterschied ist also nicht „fühlbar“ oder „erratbar“, sondern für den heute nachprüfenden Sprachanalytiker wie für die Angehörigen der homerischen Sprachgemeinschaft aus dem syntagmatischen Verhalten des Wortes erkennbar, liegt also – wie so oft bei derartigen Erscheinungen – nicht auf semantischem, sondern auf syntaktischem Gebiet.

2. Die Konstruktionen, in denen φίλος in der Ilias und der Odyssee vorliegt, lassen sich wie folgt umfassen:

1. Φίλος als attributives Adjektiv:

a. in vokativischer Anrede: γέρον φίλε Ω650, θεοὶ φίλοι ω514, φίλον θάλος X87, μαῖα φίλη ψ11 u.ö., νόμφα φίλη Γ130, δ743, φίλε ἐκυρέ Γ172, πάππα φίλ' ζ57, φίλε Φοῖβε O221, Π667. Der Voc. sg. des attributiven Adjektivs ist also φίλε, -η, -ον ¹⁾, jedenfalls nicht φίλος; im Gegensatz hierzu steht der substantivische Vokativ m. φίλος (s.u., S. 283); φίλος ὦ Μενέλαε Δ189 erweist sich also nicht nur morphologisch, sondern auch angesichts der Wortstellung als unter substantivisches φίλος gehörig.

b. sonst bei Vertretern folgender Begriffsgruppen: Körperteile und Träger der seelischen Funktionen (βλέφαρα ψ17 ²⁾, γούνατα φ55, γυῖα σ242, ἦτορ Γ31, θυμός Λ407, κεφαλή Σ114, κῆρ α310, λαιμός Τ209, μήδεα Ρ325, στῆθος θ178, χεῖρ Η130), hierzu αἰών „Alter“ Χ58 und Bekleidung (εἵματα Β261); Träger persönlicher Beziehungen, besonders auf Grund des Familienstandes (ἀκοίτης ε120, ἀκοιτῆς Γ138, ἀλοχος Σ514, ἀνάεδνος Ι288, ἀνὴρ (s.u.) Ε504, κασίγνητος Δ155, κουρίδιος ο22,

¹⁾ Auch, soweit belegt, in Verbindung mit den unter (b) aufgezählten Substantiven.

²⁾ Hier wird nur ein Beispiel für jede Verbindung gegeben; volle Beleglisten mag man aus den gebräuchlichen Homerlexika, am besten aus Ebeling's *Lexicon Homericum* ersehen.

μήτηρ β88, μήτρως B662, παῖς Π460, πατήρ Ω700, πόσις θ523, τέκνον B315, τέκος E71, τοκέυς O439, τροφός χ480, υἱός B564, υἷς δ765, nach der einen Lesart κούρη („jemandes Mädchen“ σ279), hierzu δέμνια „Ehebett“ θ277 (vgl. weiter unten), außerhäusliche Beziehungsträger (ἐταῖρος Σ233, ἑταρος T345, ξείνος „Gast“ α313); Objekte der Heimstätte und des heimischen Haushalts (δῶμα σ421, θεράπων H149, οἰκέυς E413, οἰκίον M221, πατρις (αῖα) M16), hierzu als Beziehungsbegriff νόστος „Weg in die Heimat“ Π82; Speisen (ἐνθα φίλ' ὀπταλέα κρέα ἔδμεναι Δ345); Zueignung (γέρας A167, δόσις ζ208, δῶρον Ω68) ³). Diese Nomina stellen ein in sich geschlossenes Kategorialfeld dar (s.u.).

2. a. Φίλος als prädikatives Adjektiv mit Ergänzung im Dativ, wenn das Subjekt nicht eines der φίλος auch attributiv erhaltenden Substantiva ist (Übersicht: V. 2);

b. in appositioneller Stellung mit Ergänzung im Dativ mit der gleichen Einschränkung für das Antezedens wie unter (a) (Übersicht: V. 3).

3. Substantiviertes φίλα n. pl. mit Ergänzung im Dativ (Übersicht V. 3).

4. Substantiviertes moviertes φίλος, -η ohne Ergänzung ⁴)

a. im Vokativ ohne Attribut (Übersicht S. 283);

b. in anderen Kasus nichtprädikativ m. pl. ohne Attribut oder höchstens mit Possessivpronomen oder πάντες (Übersicht VI. 1);

c. als Prädikatsnomen ohne Attribut. In Gegenüberstellung zu 2a ergibt sich demnach, daß ein adjektivisches Prädikatsnomen immer den Dativ bei sich hat, ein substantivisches nie ⁵). Dies mag auf den ersten Blick als eine willkürlich getroffene Einteilung erscheinen, indem nämlich ohne jeden weiteren Anhaltspunkt (da ja Attribute nicht vorliegen!) die Fälle mit dativischer Ergänzung als adjektivisch, die anderen hingegen als substantivisch eingereiht wurden. Jedoch ist dem nicht so, da zwischen den beiden Gruppen ein formalsyntaktisches Kriterium scheidet, nämlich die Kongruenz: bei den Fällen mit Dativ liegt grammatische Genuskongruenz vor (z.B. αἰεὶ γάρ τοι ἔρις ...

³) Nicht mitgerechnet ist Ὀδυσσεύς (τ136 ist Ὀδυσῆ ποθέουσα, φίλον κατατήκομαι ἤτορ zu interpolieren).

⁴) σ279 lesen wir nicht κούρης δαῖτα φίλοισι, sondern κούρης δαῖτα φίλησι (s. 1b).

⁵) In σε Φαίηκεσσι φίλον πάντεσσιν ἔθνηκα ν302 wird demnach der Dativ von ἔθνηκα und nicht von φίλον abhängen, vgl. jedoch V.2 unter (3).

φίλη A 177), während bei den anderen – den dativlosen Prädikatsnomina – dies nicht der Fall ist, sondern bei ihnen natürliches Geschlecht vorliegt: sie beziehen sich auf genusindifferente Personalpronomina (δός μ' ἐς Φαίηκας (bzw. Ἀχιλλῆος) φίλον ἐλθέμεν ζ327, Ω309, μή σε φίλην περ εἰσῶσαν ἐν ὀφθαλμοῖσιν ἰδῶμαι A587), auf Personalendungen der Verba (φίλος εἶη ο360, ἰκάνεις ἡμέτερον δῶ αἰδοίη τε φίλη τε Σ386, Σ425, φίλος ἔση τ254) und von Substantiven höchstens auf Namen (φίλον . . . ἐόντα . . . Μενέλαον K114, ἔσται . . . ὅτ' ἂν . . . φίλην Γλαυκῶπιδα εἴπη Θ373, φίλος ἦεν Ἰήσων μ72).

3. Φίλος läßt sich bei Homer nicht als Substantiv ansetzen, da es nirgends ein adjektivisches oder genitivisches beschreibendes Attribut regiert und seinerseits selbst oft Attribut ist. Seinem syntaktischen Verhalten und insbesondere seinen syntagmatischen Beschränkungen nach gehört es zweifellos in die Klasse der geschlechtigen Pronomina, die ja auch syntaktische Einschränkungen in genau demselben Sinne erfahren. Was wir etwa auf den ersten Blick geneigt wären, als Unterschied zwischen φίλος und den Pronomina anzusehen, ist die augenscheinliche Verbindung mit dem Dativ; diese soll weiter unten erörtert werden.

II

I. Die Substantiva, die – wenn man den Vokativen vorerst absieht – φίλος als attributives Adjektiv erhalten können (S. 265), lassen sich in ihrer Gesamtheit als Bezeichnungen der Gegenstände unveräußerlichen Besitzes charakterisieren. Dieser Satz ist umkehrbar: nicht-vokativisches attributives φίλος ist bei Homer das pronominale Ausdrucksmerkmal des unveräußerlichen Besitzes. Das hat man eigentlich schon einmal gesagt: ἐπὶ πολὺ . . . συγγενικῆς καὶ ἑταιρικῆς οἰκειότητος ἡ λέξις (scil. ,φίλος') δηλωτικῆ, ὡς μυριαχοῦ φανήσεται (Eust. 215₄₄ ad φίλα εἴματα B262); ὁ ποιητῆς ἐπὶ τῶν καθ' αἷμα συγγενῶν τὸ ,φίλος' τιθεὶς ἐξ αὐτῶν μεταφέρει καὶ εἰς ἄλλας οἰκειότητας (id. 398₄₅ ad T163).

Die grammatische Kategorie des unveräußerlichen Besitzes (possession inalienable⁶⁾ oder inséparable), nach der sich in manchen

⁶⁾ Dieser Terminus geht auf Lévy-Bruhl zurück (Baader, *Die identifizierende Funktion der Ich-Deixis im Indoeuropäischen* 14). C. C. Uhlenbeck, *Het identificerend karakter der possessieve flexie in talen van Noord-Amerika* (Versl. en Mededeel. der Kon. Akad. van Wetensch. V/2, 1917, 345ff.) gebraucht neben „onvervreemdbaar“/„vervreemdbaar“ auch die Termini „intieme“/„uiterlijke possessie“.

Sprachen die in diese Klasse gehörigen Substantiva syntaktisch von Substantiven ohne Possessorausdruck einerseits und von solchen, die veräußerbares Gut bezeichnen, andererseits unterscheiden, ist bisher mehr in außerindogermanischen als in indogermanischen Sprachsystemen untersucht worden. In denjenigen Sprachen, in denen die hier angedeutete Dreiteilung (Nichtpossession – Alienabilität – Inalienabilität) besteht, ist die Einteilung naturgemäß nicht immer identisch, es lassen sich jedoch gewisse Sachgruppen angeben, die häufig als Inalienabilia in denjenigen Sprachen wiederkehren, in denen morphologische oder syntaktische Kennzeichen der Inalienabilien vorliegen. In Sprachsystemen, in denen die Inalienabilia von den Alienabilien nicht formell unterschieden sind, muß ein inalienabler Gegenstand entweder zu den nichtbesessenen oder zu den alienablen Gegenständen gereiht werden, da eben nur diese Dichotomie existiert. Die Einreihung ist nicht zwangsläufig in allen Sprachsystemen dieselbe, und so gelangen wir zum bekannten Kennzeichen des Inalienabelbegriffes: Bezeichnungen von Inalienabeln sind häufig diejenigen Substantiva, die in dem einen Sprachsystem obligatorisch mit Possessivpronomina, in dem anderen ohne dieselben konstruiert werden: *er schüttelte den Kopf* – *he shook his head*, *er hat Geburtstag* – *he has his birthday*, *ich will das Fleisch aufessen* – *I want to finish my meat*.

Häufig als Inalienabilia ausgedrückte Bezeichnungsgruppen sind bekanntlich die folgenden 7):

1. Körperteile (im Ungarischen, Melanesischen, Fidji, Sioux, Fox, Haida, Keltischen, Nehwatl, Koptischen usw.), vgl. oben *den Kopf* gegenüber *his head*.
2. Teilbegriffe oder Gesamtheitsbegriffe (im Ungarischen, Melanesischen, Koptischen, Israelisch-Hebräischen);
3. Raumbeziehungen wie „rechts“ und „links“ (im Melanesischen, Israelisch-Hebräischen);

7) Vgl. die von Baader, *o.c.* 13 gegebene Übersicht. Die hierunter aus einzelnen Sprachen angeführten Daten ersehe man: für das Ungarische aus Simonyi, *Die ungarische Sprache* 260; für das Melanesische (auch Mota) aus Lévy-Bruhl, *MSL* 19, 1914, 96ff.; für Fidji aus Benveniste, *BSL* 48, 1952, I. XXIIIff.; für nordamerikanische Dialekte aus Uhlenbeck, *o.c.*; für Algonquin und Keltisch aus Havers, *WuS* 12, 1929, 169ff.; für Nehwatl aus Schömb, *Aztekische Schriftsprache* 29; für das Koptische aus Steindorff, *Koptische Grammatik* §§ 84, 166; für das Israelisch-Hebräische aus meinem Aufsatz, *BSL* 53, 1957–58, I. 321 ff.

4. Verwandtschaftsnamen (im Ungarischen, Melanesischen, Yuki, Sioux, Fox, Haida, Fidji, Algonquin, Nehwatl, Israelisch-Hebräischen etc.);

5. Speisen und Getränke (Mota, Algonquin), vgl. oben *das Fleisch* gegenüber *his meat*;

6. Kleidungsstücke und Hausrat (im Keltischen, Lamutischen, Algonquin, Fox, etc.);

Wie das homerische Griechisch in diese Übersicht einzureihen wäre ergibt sich aus der oben für die Regentia von attributivem φίλος angegebenen Einteilung von selbst. In Bezug auf das Homerische sollen diese Substantiva nunmehr „Inalienabilia“ genannt werden ⁸⁾.

2. In manchen Sprachsystemen lassen sich zwei Untergruppen der Inalienabilia unterscheiden:

A. Substantiva, welche die formellen Merkmale der Inalienabilia ständig an sich tragen: meistens Bezeichnungen solcher Gegenstände, die ebensowenig von ihrem „Besitzer“ zu trennen sind wie der Besitzer von ihnen und die, was sie sind, erst dadurch werden, daß sie es in Bezug auf etwas sind ⁹⁾, Dinoysios Thrax' Nominalklasse der ὀνόματα πρὸς τι ἔχοντα, wie Verwandtschaftsnamen, „rechts“, „links“ u.a.; dies sind die Substantiva, die in den Sprachen mit Dichotomie des semantischen Spektrums auf dem Gebiete der Possession teils zu den „besessenen“ teils zu den „unbesessenen“ Begriffen gezählt werden (oben unter 1). Das Homerische steht als dreigliedertes System einem dichotomischen späteren System des Griechischen gegenüber; daher tritt im klassischen Griechisch bei diesen Inalienabilien anstelle des φίλος eher ein Artikel als ein Possessivpronomen (Typus τὸν πατέρα φιλεῖ „amat patrem, er liebt seinen Vater“).

B. Substantiva, die manchmal die Formcharakteristiken der Inalienabilien tragen, manchmal jedoch ohne dieselben auftreten, wodurch dann Begriffspaare wie die folgenden morphologisch-syntaktisch – und

⁸⁾ In keiner Sprache sind zwangsläufig alle Bezeichnungen von Körperteilen, Familienmitgliedern etc. Glieder der morphologisch-syntaktischen Klasse der Inalienabilien und tragen automatisch deren formelle Kennzeichen; z.B. in manchen Sprachen gehört „Frau“ nicht in die genannte Klasse. Die Zugehörigkeit der Inalienabelnklasse ergibt sich für uns aus formellen (z.B. homerisch φίλος), nicht aus real-sachlichen Kriterien.

⁹⁾ Lévy-Bruhl, o.c. 99, sagt über den Kopf: „je sens qu'elle n'est pas sans moi, ni moi sans elle“; Uhlenbeck, o.c. 350, definiert den Vater als „de persoon die tot mij in de verhouding van vader staat“.

nicht, wie bei possessionsdichotomischen Sprachen, lexikalisch – unterschieden werden:

„Haus“ mit Merkmal der Inalienabilien / „Haus“ mit Merkmal der Alienabilien = (in lexikalischem Ausdruck) „home“ / „house“¹⁰;

„Mann, Frau, Knabe“ mit Merkmal der Inalienabilien / dieselben gekennzeichnet als Alienabilia = „husband, wife, Sohn“ / „man, woman, Knabe“, vgl. *my boy* / *the boy*, *mein Mann* / *der Mann* u.ä.;

„Fleisch“ (inalienabel) / „Fleisch“ alienabel = Fleisch als Körperteil / Fleisch als Speise oder Ware¹⁰), oder = Fleisch als Speise / Fleisch als Ware, vgl. *flesh* / *meat*;

„Kopf“ (inalienabel) / „Kopf“ (alienabel) = Körperteil / Ware beim Metzger¹¹); inalienables „Holz“ = Hausrat / Holz als Ware¹²); etc.

Solche Unterscheidungsmöglichkeiten scheinen auch vom homerischen Griechisch nicht ungenutzt gelassen worden zu sein. Die weiter unten näher zu erörternden syntaktischen Eigentümlichkeiten der Inalienabilien – vornehmlich der Gebrauch von φίλος, die Ausschaltung des adnominalen possessiven Genitivs des Personalpronomens und die Ausschaltung des Determinativpronomens ὁ – gestatten in der homerischen Sprache zu unterscheiden zwischen:

mit Inalienabilitätsausdruck	mit Alienabilitätsausdruck (evtl. auch ohne Inalienabilitätsausdruck)
ἀνὴρ „jemandes Mann“, i.e. 1. „Gatte“: ἀνδρὶ φίλῳ ἐλθόντι γανύσσεται Ε504, Ἐριφύλην, ἣ χρυσὸν φίλου ἀνδρὸς ἐδέξατο τιμήμεντα λ327; 2. „einer von jemandes Leuten“: ὅθι τοὶ φίλοι ἄνδρες ἐγγεγάασι Δ41, ἣ φίλον ἄνδρα διωκόμενον περὶ τεῖχος . . . ὀρώμαι X168, φίλῳ ἀνέρος υἱὸς ἐμὸν δῶ ἔκειθ' δ169, vgl. γ237.	„Mann, Mensch“ passim, z.B. mit Determinativpronomen: τῶν ἀνδρῶν, οἱ ἄστυ περὶ Πριάμοιο μάχοντο ε106.

¹⁰) Israelisch-Hebräisch (BSL 53, 1957–58, I. 322), Algonquin (Uhlenbeck, o.c. 354).

¹¹) Das geht für Algonquin aus Bloomfield's Besprechung von Havers' *Handbuch der erklärenden Syntax* hervor (*Language* 10, 1934, 37).

¹²) Lamutisch (Benzing, *Lamutische Grammatik* 81).

δῶμα „Heim“: τοῦ γὰρ φίλον ἵκετο δῶμα σ421; vgl. σ270, σ428, κ434;	„Wohngebäude“: κτῆσιν ἐμὴν δμῶάς τε καὶ ὑψέρεφες μέγα δῶμα T333; οἱ ἐποίησαν θάλαμον καὶ δῶμα καὶ αὐλήν Z316;
παῖς „jemandes Junge (Mädchen)“, „Sohn, Tochter“: παῖδα φίλον τιμῶν Π460 u.ö.; sehr charakteristisch ist der Vers Z466: ὥς εἰπὼν οὗ παιδὸς ὀρέξατο φαίματος Ἐκτωρ fortgesetzt durch:	„Knabe, Mädchen“ ἀψ δ' ὁ παῖς πρὸς κόλπον . . . τιθήνης ἐκλίνθη Z467;
ξεῖνος „Gast“: οἷα φίλοι ξεῖνοι ξείνοισι διδοῦσιν α313, μνήμα ξείνοιο φίλοιο κέσκετ' ἐνὶ μεγάροισι φ40;	„Fremder“: ὁ ξεῖνος passim;
δῶρον „das jemandem zugeeignete Geschenk“: φίλα δῶρα, τά μοι . . . Οὐρανῖωνες ἄλβια ποιήσεται ν41;	„die von jemandem dargebrachte Gabe“: τὰ . . . δῶρ' Ἀφροδίτης Γ54, μὴ . . . τὰ δῶρα οὕτω χρῆζοντι κολούετε λ339;
εἵματα „Kleider“: ἅτάρ τοι εἵματ' ἐνὶ μεγάροισι κέονται X510;	„Kleidungsstücke“: λυγρὰ εἵματα „Trauerkleidung“ ρ203, ω158 u.ö.

III

1. Festzustellen ist nunmehr, was die syntaktische Funktion von φίλος in Bezug auf die mit ihm verbundenen Substantiva ist. Die Beschreibung dieser Funktion als der eines „(reflexiven) Possessivadjektivs“ ist nicht zutreffend, da man nicht behaupten darf, φίλος übe in Bezug auf die Inalienabilienbezeichnungen dieselbe Funktion aus wie etwa ἐμός, σός, ὅς bei anderen Substantiven, zumal ἐμός, σός, ὅς auch bei den Inalienabilienbezeichnungen auftreten. Wir haben vielmehr diejenigen Pronominalformen aufzusuchen, deren Auftreten demjenigen von φίλος komplementär ist, mit anderen Worten, die von der Verbindung mit Inalienabilienbezeichnungen ebenso ausgeschlossen sind wie attributives φίλος von derjenigen mit nicht in diese Klasse gehörigen Substantiven.

Ist der Besitzer eines Gegenstandes durch ein Personalpronomen bezeichnet, so können in der possessiven Konstruktion einer oder mehrere der folgenden Komponenten auftreten:

A. das Determinativpronomen ὅ;

B. 1. ein persönliches oder reflexives Possessivadjektiv, oder 2. der Possessivkasus eines Personalpronomens;

C. eine mit B genus-numerus-kongruente Genitivform des kontrastiven Identifikationspronomens ¹³⁾ αὐτός.

Die Verbindungsmöglichkeit der genannten Komponenten ist bei Alienabilien und Inalienabilien nicht die gleiche:

Ausgeschlossen sind bei Homer die Konstruktionen A + C, A+B₂ + C, d.h. die Formtypen ,ὁ ἵππος αὐτοῦ', ,ὁ μευ ἵππος αὐτοῦ'.

Die folgenden Konstruktionen finden sich bei den Inalienabilienbezeichnungen wie bei anderen Substantiven, wobei zu berücksichtigen ist, dass der adnominale Possessivkasus der Personalpronomina bei den Inalienabeln der Dativ, bei den anderen Substantiven hingegen gewöhnlich der Genitiv ist (s.u.):

	Alienable	Inalienable
B ₁	z.B. ἐμὸν . . . ἔγχος Z126, κτεάτεσσιν ἐοῖς α218	z.B. ὑπὸ βλεφάροισιν ἐμοῖσιν Ω637, εἰς ἐμὰ δέμνια βάντες Θ314
B ₂	z.B. μευ βουλέων A273, σφεων πολέες γ134	z.B. δηρόν μοι αἰών Ι415, οἱ κῆρ ὥρμαινεν φρεσὶ ῥσιν σ344
A+B ₁	z.B. τὰ σὰ τεύχεα Π40, τὰ δ' . . . τεύχεα P193	z.B. τὰ σὰ γούνατα γ92, δ322, τὸ σὸν γέρας Α185, τὸ ἐμὸν κῆρ Z523, πατὴρ οὐμός Θ360
B ₁ +C	σῶ αὐτοῦ κράτι χ218, vgl. α7, Ο39, Z446, β45, α409, δ643, π196 – 197	σῆς αὐτοῦ κεφαλῆς ο262, ὑμέτερος θυμὸς αὐτῶν β138, ἐῶ αὐτοῦ θυμῶ Κ204 – 205

Nur bei den Alienabilienbezeichnungen sind belegt die Konstruktionen:

C 14)	z.B. αὐτῶν . . . κτήματα ρ532
A+B ₁ +C	τὰ σ' αὐτῆς ἔργα α356, φ350, Z490, vgl. ξ 185
B ₂ +C	σέο αὐτοῦ κήδεα λυγρὰ λ369, ἔο αὐτοῦ πάντα θ211, vgl. T302, 384.

Die Konstruktion A, d.h. das Determinativpronomen ὁ ohne weitere Bezeichnung eines Possessors, ist belegt bei den Alienabeln (Typus ὁ ἵππος) sowie bei den Bezeichnungen von Blutsverwandten, die aus

¹³⁾ Wagon, *Le pronom d'identité* 1 et passim.

¹⁴⁾ Αὐτοῦ „illico“ in τίς μιν θεὸς ὥρορεν, ἥ ἐ καὶ αὐτοῦ θυμὸς ἐφωρμήθη ἔμεν ἐς Πύλον δ712; σ354–355 ist mit einhelliger Überlieferung αὐτοῦ καὶ κεφαλῆς und nicht καὶ κεφαλῆς zu lesen.

der Inalienabelnklasse auch sonst durch eigenes syntaktisches Verhalten hervorstechen (s.u.), z.B. ἡ δέ νυ μήτηρ X405, τῆς μητρός Φ412, τὸν υἱόν Δ399, τοῦ πατρός π149, nicht aber bei sonstigen Inalienabilienbezeichnungen, vgl. III.5.

Demgegenüber findet sich nur bei den Inalienabilien die Form $A+B_2$ (μοι τὸν παῖδα... ἐξαγάγοις T331, σοὶ τὸ γέρας πολὺ μεῖζον A167, οἱ τοῖς ἄλλοις ἐτάροις, οἱ ἅμ' αὐτῷ ἔποντο τ197) sowie die folgenden Verbindungen mit φίλος:

φίλος allein: z.B. φίλοις ἐτάροισι ι466, κατεπλήγη φίλον ἦτορ Γ31, φίλον ἀπὸ θυμὸν ἐλοίμην ξ405, φίλους ἐκλαίον ἐταίρους μ309, auch bei Bezeichnungen der Blutsverwandten: πατρὶ φίλῳ καὶ μητρὶ Ξ502, μαρναμένον φίλον υἱόν Γ307;

φίλος + B_1 : z.B. σὸν... φίλον ἦτορ O182, ἐμὸν δ' ἐγέλασσε φίλον κῆρ ι413, σὸν... φίλον κῆρ π274, πατρός ἐοῖο φίλοις ξ177, ἀμφὶ δ' ἐὼν φίλον υἱόν E314, δν φίλον υἱόν Z474;

φίλος + B_2 : z.B. γήθησε... μοι φίλον ἦτορ η269, οἱ φίλον ἦτορ E670, Σκύρῳ μοι ἔνι τρέφεται φίλος υἱός T326, τὸν... οἱ... φίλοις ἐπὶ γούνασι θῆκε τ401, τῷ μοι φίλα γυῖα λέλυνται θ233, οἱ φίλος ὤλεθ' ἐταῖρος P642, μοι φίλος ἦθελε θυμός ν40.

Über das nur bei Blutsverwandtschaftsnamen auffindbare φίλος mit substantivischem Genitiv s. III. 3.

2. Hält man die Konstruktionen

$\left. \begin{array}{l} \text{φίλος} \\ \text{φίλος} + B_1 \\ \text{φίλος} + B_2 \end{array} \right\} \text{ bei den Inalie-} \\ \text{nabilien}$

gegen die Konstruktionen

$\left. \begin{array}{l} C, A \\ A + B_1 + C \\ B_2 + C \end{array} \right\} \text{ bei den anderen} \\ \text{Substantiven}$

– da diese Paare im Komplementärverhältnis stehen –, so ergibt sich die Äquivalenz von φίλος mit A als auch mit C, d.h. φίλος ist besitzeridentifizierend-determinierend.

Andererseits scheint die nur bei Inalienabilien vorkommende Konstruktion $A + B_2$ darauf hinzuweisen, daß bei dieser Klasse, abgesehen von Blutsverwandtenbezeichnungen, Determination wie auch Identifikation ohne gleichzeitige Possessorbezeichnung ausgeschlossen ist.

Hieraus ergibt sich, das φίλος soviel wie (δ) αὐτοῦ heißt ¹⁵⁾, wozu

¹⁵⁾ Zu der in Bezug auf Inalienabilia oft behandelte Funktionsgleichheit zwischen Besitzerbezeichnung und Identifikation-Determination, auf die wir hier nicht einzugehen brauchen, vgl. hauptsächlich Baader, o.c. 61ff. (im Anschluss an Lévy-Bruhl und Uhlenbeck). — Im Zusammenhang mit der Stel-

noch hinzuzufügen ist, dass φίλος genau wie αὐτοῦ den Gebrauch eines das substantivische Regens beschreibenden Adjektivs nicht zuläßt (vgl. VI.1): eine Konstruktion, φίλος ἐὺς υἱός' findet sich bei Homer ebenso wenig wie, (ἐμὸς) αὐτοῦ ἐὺς ἔππος' (in φίλη πατρίς αἶα ist πατρίς αἶα das possessiv bestimmte inalienable Regens, nicht πατρίς beschreibend zu einem etwaigen Regens αἶα, das wohl nicht inalienabel wäre). Ebenso gibt es auch nicht den Konstruktionstypus, ἔππος τοῦ δεῖνα

lung von φίλος als Identitätspronomen des unveräußerlichen Besitzes möchte ich auf eine Parallelerscheinung in einer anderen idg. Sprache aufmerksam machen. (Die Kenntnis der nachfolgenden Tatsachen verdanke ich der Güte Herrn Professors H. G. Güterbock, der mir die Einsicht in manche der verwickelten syntaktischen Erscheinungen des Hethitischen ermöglichte.) Die heth. Pronominalform *za* ist im Wesen reflexiver Natur und erlaubt den Ausdruck von Gegensätzen wie „sich waschen“/„waschen“, „an sich bringen“/„(von einem Ort an den anderen) „bringen“ (s. Fußnote 25) u.ä. (Friedrich, *OLZ* 39, 1936, 304 f., und *Heth. Elementarbuch* I. 75). Es sind wohl zwei Fälle zu unterscheiden: 1. *za* vertritt ein Personalpronomen des Dat.-Acc.; 2. *za* tritt an ein Personalpronomen heran. Im letzteren Fall ist es wahrscheinlich auf den Dativ des Personalpronomens beschränkt (vgl. die Syntagmatik von φίλος), da das Pronomen der 2. sg. vor *za* eine eigene Flektionsform besitzt (vgl. Götze-Pedersen, *Muršilis Sprachlähmung* 82, die allerdings nicht an eine Dativform denken). *Za* steht aber auch sonst in einer der des Havers'schen „sympathetischen“ Dativs ähnlichen Gebrauchsweise, wie „er legt sie sich (*za*) in seinen (*ši*) Mund“, „die zwei Opfermandanten gießen sich (*za*) das Wasser über ihre (*ŠU.NU*) Köpfe“, in denen der Unterschied etwa zu *caput illi perfunde* (Petr. *Sat.* 64) nur darin besteht, daß das Possessivverhältnis im Hethitischen anscheinend doppelt („pleonastisch“, Friedrich, *OLZ*, l.c.) ausgedrückt ist. Um eine solche pleonastische Auffassung zu vermeiden, genügt es, dem *za* nicht besitzeridentifizierende, sondern bloß bezugsausdrückende Funktion zuzuschreiben, sodaß also *za*+*ši* (Possessivsuffix) = ol („sympathetischer“ Dativ) wäre. Mit anderen Worten, *za* wäre die Partikel der untrennbaren Beziehung oder der durch den „Besitzer“ erfolgten Identifikation. Dies mag daraus hervorgehen, daß Konstruktionen der angeführten Art mit den folgenden Besitzobjekten belegt sind: Körperteile, „Person“, „Land“ (= „Heimat“), Familienmitglieder, „Platz“, „Haus“, „(Reise-) Weg“. Wie weit es also auch mit der hier vorgeschlagenen Definition des *za* in derartigen Konstruktionen seine Richtigkeit haben mag, so geht jedenfalls aus der Beschränkung einer syntaktischen Konstruktion auf eine durch die genannten Substantiva charakterisierte Wortklasse deutlich die Existenz einer formalen Kategorie „unveräußerliches Besitz-(Bezugs-)verhältnis“ hervor (vgl. Fußnote 17). Charakteristisch ist, daß in Parallelkonstruktionen das *za* beim inalienabeln, nicht aber beim alienabeln Objekt steht („die Mutter führte ihr Kind“ hat ein *za*, „das Schaf leitete sein Lamm“ hat keines; vgl. Friedrich, *OLZ*, l.c.).

αὐτοῦ' bei Homer, und das Gleiche gilt — von den sogleich zu besprechenden Formen abgesehen — auch für ,φίλη χείρ τοῦ δεινα'.

3. Die im vorigen Paragraphen angedeutete Erscheinung, daß im homerischen Griechisch Determination einer Inalienabilienbezeichnung (mit Ausnahme der die Verbindung mit dem nackten $\acute{o}$ zulassenden Blutsverwandtenamen) die Besitzanzeigung notwendig macht, ist nichts weiter als ein Einzelfall einer der in der Syntax der Inalienabilien bestbekannten Erscheinungen, nämlich der „fixen Possessivverbindungen“, d.h. das Nichtauftreten eines Inalienabiliennamens ohne Possessorbezeichnung, besonders in determiniertem Zustande. In manchen Sprachen gibt es nicht eine Form wie „Vater“, sondern nur „mein, dein etc. Vater“¹⁶⁾; eine Folge dieser syntaktischen Eigentümlichkeit ist es, wenn ein determiniertes Regens mit besitzanzeigendem attributiven Substantiv sodann eine Konstruktion des Typus „seine Hand des Mannes, seine Frau des N“ eingeht¹⁷⁾, wobei das regierte Substantiv zu dem nunmehr sogenannten proleptischen Possessivpronomen epexegetisch in Apposition tritt, genau so wie wir es (mit dativischem Adnominal) in *dem N sein Vater, son père à N* haben.

¹⁶⁾ Algonquin, Ungarisch (nicht absolut gültig! vgl. Simonyi, *l.c.*) etc., laut Havers, *WuS* 12, 1929, 161–172; zu syntaktischer Sonderbehandlung der Verwandtschaftsnahmen (innerhalb der Inalienabelnklasse) vgl. Uhlenbeck, *o.c.* 352.

¹⁷⁾ Koptisch (Steindorff, *o.c.* § 166), Israelisch-Hebräisch (*BSL* 53. I. 323); Havers, *WuS* 12, 1929, 169 zitiert hierzu die altirische Konstruktion „damit ich abschlage sein Haupt von dem unverschämten Kerl“. Doppelter Ausdruck des „Besitz“verhältnisses liegt auch in den hethitischen Texten dann vor, wenn statt des bloßen Bezugspronomens *za* (Fußnote 15) ein richtiges besitzanzeigendes Fürwort im Genitiv an ein bereits mit Possessivsuffix versehenes Substantiv herantritt, z.B. „mein (*amel* und nicht bloß *za*) Gemahl von mir“ etc. Laut Hrozný, *Sprache der Hethiter* 99, 108, 111, 114, liegen in diesen Konstruktionen folgende Substantiva vor: „Gemahl“, „Schwester“, „Sohn“, „Land“, „Person“, „Vater“, „Sklave“, „Frau“, „Kind“, „Schutzgott“. Es muß zwar darauf hingewiesen werden, daß alle diese Wörter mit Akkadogrammen geschrieben werden und auch das Possessivsuffix in seiner akkadischen Form in das Akkadogramm miteinbegriffen ist, sodaß die Möglichkeit besteht, daß das akkadische Suffix keinem realen hethitischen Lautwert entspricht. Diese theoretisch bestehende Möglichkeit ist aber unwahrscheinlich, da hier wiederum (vgl. Fußnote 15) durch die Abgrenzung einer syntaktischen Erscheinung auf eine charakteristische Substantivklasse die Existenz der grammatischen Kategorie „unveräußerliches Besitzverhältnis“ für das Hethitische erwiesen zu sein scheint.

Bei den typischsten Vertretern der Klasse der *ὀνόματα πρὸς τι ἔχοντα*, nämlich den Blutsverwandtenbezeichnungen, liegt dieser Konstruktionstypus auch im Homerischen vor: 'Ιθαίμενεος φίλον υἱόν „dem Ithaimenes seinen Sohn" Π586, ebenso:

φίλον υἱόν 'Οδυσσεύς θεοιο γ398, Χάλκωνος φίλον υἱόν Π595, Αἰσυήταο... φίλον υἱόν Ν427, Κοπρῆος φίλον υἱόν Ο639, 'Αδμήτου φίλος υἱός Ψ289, 'Οδυσσεύς φίλος υἱός γ352 u.ö., φίλον υἷα... 'Ιδομενῆος ν259, φίλος υἱός 'Οδυσσεύς θεοιο ρ3 u.ö., φίλον υἷα... 'Ιδομενῆος ν259, τῶν ἀνδρῶν φίλοι υἱες β51; πατὴρ ἐοῖο φίλον μήτρωα Β662; Πριάμοιο φίλος παῖς Η44, παῖδα φίλον Μελανῆος ω103, Αὐτολύνκοιο παῖδες φίλοι τ455, παιδὸς... Νεοπτολέμοιο φίλοι λ506; πατέρ' Αὐτόλυκον μητρὸς φίλον ω334, Τηλεμάχοιο φίλον πατέρα Δ354.

4. Schwerer zu beurteilen ist der syntaktische Status der zweimal belegten Konstruktion des anaphorischen τοῦ mit φίλος: τοῦ γὰρ φίλον ἔκετο δῶμα „zu dem seinem Haus" σ421, τοῦ δ'οὐκ ἔθεται φίλον ἦτορ „dem sein Herz" Ο166¹⁸⁾.

5. In Anbetracht des Ausschlusses des determinativen ὁ von der Verbindung mit Inalienabilienbezeichnungen, soweit es nicht von einer Possessorbezeichnung gefolgt oder auf eine Blutsverwandtenbezeichnung bezogen ist, ist noch auf folgende Einzelheiten einzugehen:

An den in II.2 angeführten Stellen mit ὁ ξεῖνος, ὁ ἀνὴρ, τὸ δῶμα, τὸ δῶρον steht ὁ nicht bei einer Inalienabilienbezeichnung, sondern dient geradezu zum Ausdruck zur syntaktischen Differenzierung des alienabeln Begriffes: „Fremder" (und nicht „Gast") usw. Bei der öfters auftauchenden Phrase τὸ γὰρ γέρας ἐστὶ γερόντων (θανόντων) Δ323 u.ö., steht τό nicht adnominal zu γέρας, sondern als Subjekt zu γέρας γερόντων, wie aus der Parallelkonstruktion ὁ γὰρ γέρας ἐστὶ θανόντων Ψ9 u.ö. deutlich hervorgeht. Ξ272 zeigt die Konstruktion χειρὶ... τῇ ἐτέρῃ μὲν..., τῇ δ'ἐτέρῃ... eindeutig, dass nicht χεῖρ determiniert, sondern τῇ ἐτέρῃ μὲν... τῇ δ'ἐτέρῃ kontrastiert ist; dasselbe gilt für das appositionelle πατήρ zu ὁ μὲν Ε352, kontrastiert Ε354 durch ὁ δέ. Κ235 schließlich ist die Lesart τῶν... ἔταρον vorzuziehen.

¹⁸⁾ In ἡ μάλα μευ καταδάπτει ἀκούοντος φίλον ἦτορ π92 wird man wohl μευ ἀκούοντος als Gen. abs. auffassen müssen (zu dieser Stelle vgl. auch Havers, *Untersuchungen zur Kasussyntax* 69, 96); in μευ φθινύθουσι φίλον κῆρ ἀμφ' ἐμ' ὀδυρόμενοι κ485-486 wird man wohl μευ φθινύθουσι „verzehren um mich (ihr Herz)" verbinden („mein Herz" hätte weniger Sinn).

Μοι δὲς τὴν χεῖρ' Ψ75 ist trotz der irreführend leicht zu bewerkstellenden deutschen Übersetzung „gib mir die Hand“ doch nicht so leicht zu analysieren, wenn man tiefer schürft: „geben“ soll wohl bedeuten, einen Gegenstand aus dem Besitz des einen in den des anderen überzuführen, was hier wohl kaum zutrifft und sei es nur aus dem Grunde, daß eine Hand inalienabel, d.h. ihr Besitz unüberführbar ist. Vielleicht hat χεῖρ hier eine metaphorische, alienable, Bedeutung, denn formell ist ja die Konstruktion μοι τὴν χεῖρα bei Homer gebräuchlich, nur müsste dann χεῖρ als „meine“ und nicht die des Angeredeten aufgefaßt werden, wie man ja auch beim französischen *il m'a serré la main* nicht ausgedrückt findet, von wessen Hand eigentlich die Rede ist. Was immer auch die semantische Grundlage dieses dem Griechischen mit einigen anderen Sprachen gemeinsamen Idioms auch sein mag ¹⁹⁾, steht die hier erscheinende Hand schon zumindest dem Wortlaut (δὲς ²⁰⁾!) nach außerhalb der Inalienabelnklasse, und die Konstruktion daher nicht im Widerspruch zu der hier aufgestellten syntaktischen Regel.

6. Die Erkenntnis, daß der bei den Inalienabilienbezeichnungen stehende adnominale Possessivkasus hauptsächlich der Personalpronomina der Dativ ist, verdanken wir den unüberholbaren *Untersuchungen zur Kasussyntax der indogermanischen Sprachen* (1911) von Wilhelm Havers. Hier werden zur Klassifizierung des Gebrauches des von Havers so genannten „Dativus sympatheticus“ Begriffskategorien aufgestellt, die sich mit den hier für φίλος angesetzten fast vollkommen decken. Obwohl der Terminus „possession inaliénable“ von Lévy-Bruhl ²¹⁾ drei Jahre später eingeführt wurde und Havers später eine sehr große Vorliebe zur Behandlung der untrennbar besessenen Objekte zeigte — man braucht nur die Häufigkeit seiner Heranziehung in unserer allgemeinsprachwissenschaftlichen Besprechung ins Auge zu fassen! — sehen wir nie in seinen Schriften auch nur eine Andeutung der Identität der für die „fixen Possessivverbindungen“

¹⁹⁾ Vgl. Fußnote 26.

²⁰⁾ Es ist in einem Sprachsystem auch möglich, die Begriffe „transfer, give, what is not inherently one's own, give something (e.g. a person) not normally considered capable of material transfer“ und „give that which is inherently one's own or which it is one's right to give“ lexikalisch zu unterscheiden (Longacre, *Lang.* 34, 1958, 487).

²¹⁾ S. Fußnoten 6, 7.

(III. 3) maßgebenden Begriffsgruppen mit denjenigen, die er selbst für den Dativus sympatheticus aufgestellt hatte, obwohl diese Identität auf der Hand liegt und in die Augen springt; und so bleibt auch der Terminus „possession inaliénable“ seinen Schriften ferne. Man sollte hier wohl nicht an mangelnde Aufmerksamkeit oder an Nachlässigkeit Havers' denken, sondern viel eher daran, daß — wie er in allen Behandlungen der Frage betont ²²⁾ und wofür er die Vorwürfe mancher Kritiker einstecken mußte ²³⁾ — nach ihm die „fixen Possessivverbindungen“ Kennzeichen „primitiver“ Sprachsysteme sind ²⁴⁾; so scheint es, daß er sich weigerte, diese „primitiven“ syntaktischen Erscheinungen in irgendeinem Bezug mit den im Griechischen, Lateinischen, Altindischen usw. beobachteten in Verbindung zu setzen. Hätte er dies getan, so wäre ihm auch die Eigenheit der Verbindung mit φίλος gerade in dieser Begriffsgruppe wohl nicht entgangen.

Hervorzuheben ist, daß wir für φίλος eine Gruppe von Wörtern, nicht von Gegenständen, angesetzt haben, und obgleich sich diese Wörter bequem in Begriffsgruppen einteilen lassen, ist nicht gesagt, dass alles, was lexikalisch unter diese Sachgruppen fallen könnte, auch dieselben syntaktischen Eigentümlichkeiten haben müßte. Unsere Wortklassen bilden eine durch die Verbindungsmöglichkeit mit φίλος geschaffene syntaktische Kategorie, und naturgemäß haben alle syntaktischen Feststellungen nur innerhalb formalsyntaktischer Kategorien (und nicht in Sachgruppen!) ihre Giltigkeit.

Andererseits ist die hier angesetzte syntaktische Kategorie um die „Begriffe der Zueignung“ weiter als diejenige Havers' für den Dativus sympatheticus. Bekanntlich bekommen auch die Wörter δῶρον, δόσις, γέρας sehr häufig den Dativ, aber wir sind eher geneigt, in der Konstruktion δῶρόν τι διδόναι im Dativ eines der vom Verbum regierten Objekte zu sehen. Aber eine Reihe von Umständen weisen darauf hin,

²²⁾ WuS, I.c.; *Handbuch der erklärenden Syntax* 111.

²³⁾ Bloomfield, *Lang.* 10, 1934, 36f.

²⁴⁾ Auch nach Bang, *Le Muséon* 36, 1923, 193, ist das Vorhandensein solcher Verbindungen im Türkischen ein Zeichen dafür, daß sich der Türke noch nicht dazu emporgerungen hätte, „eine Nase als Abstraktum anzusehen“. In einer ähnlichen Feststellung Uhlenbeck's (o.c. 358) steht jedoch „primitiv“ passend in Anführungszeichen: „... begrippen, die voor de ‚primitieve‘ mentaliteit ... moeilijk anders dan in betrekking tot een bepaalden persoon kunnen worden gedacht.“

daß dies zumindest ursprünglich nicht so gewesen sein muß. Bekannt ist, daß das Altindische in diesen Konstruktionen häufig nicht den Dativ, sondern den Genitiv aufweist, was auf den possessiven Charakter des Kasus hinweist. Ferner sei darauf hingewiesen, daß zwischen $\delta\omega\rho\acute{o}\nu \tau\iota\upsilon$ und $\delta\omega\rho\acute{o}\nu \tau\iota\upsilon\omicron\varsigma$ bei Homer ein Gegensatz zwischen „die jemandem zugeeignete“ und „die von jemandem dargebrachte Gabe“ zu bestehen scheint (II.1). Hauptsächlich aber ist zu erwähnen, daß in den älteren indogermanischen Sprachsystemen die Begriffe „geben“ und „nehmen“ etymologisch zusammenfallen²⁵⁾; so ist also einerseits zu erwarten, daß der von Havers, *Untersuchungen*, passim, für die „Verba des Wegnehmens“ ($\alpha\varphi\alpha\iota\rho\epsilon\acute{\iota}\nu \tau\acute{\iota} \tau\iota\upsilon$, Havers' Kategorie IV) angesetzte Gebrauch des Dativus sympatheticus auch bei den „Verben des Gebens“ aufzufinden sei, und daß andererseits die etymologische Grundbedeutung — und vielleicht auch frühe Funktion — von Verben wie $\delta\iota\delta\acute{o}\nu\alpha\iota$ (die hethitische Parallelwurzel wird unpassend „nehmen“ übersetzt!) eigentlich „übertragen“ ist, was zwei Konstruktionen ermöglicht, nämlich: 1. „aus jemandes Besitz übertragen“²⁶⁾, 2. „in jemandes Besitz übertragen“ — die letztere mit possessivem adnominalen Dativ zum Ausdruck des nunmehrigen Besitzers.

Beschränkt man andererseits die hier zur Erörterung stehende syntaktische Kategorie auf ihre formelle Grundlage (d.h. die angesetzten Wörter und nicht Sachgruppen), so fallen auch praktisch alle Ausnahmen weg, die Havers bei seiner Regel, diese Begriffsbezeichnungen verbünden sich mit dem Dativus sympatheticus der Personalpronomens und nicht mit dessen Genitiv, zulassen mußte²⁷⁾. In drei Fällen besteht das Regens des possessiven Attributes aus mehreren koordinierten Wörtern, von denen nur eines in unsere Wortkategorie fällt, sodaß der auf alle Substantiva bezogene Adnominal nicht der nur auf eines der Substantiva passende Dativ zu sein braucht: $\mu\epsilon\upsilon$

²⁵⁾ Altindisch *dadāti* „gibt“, *ā-datte* „nimmt“, hierzu $\delta\iota\delta\omega\sigma\iota$, *dat* gegenüber heth. *dāi* „überträgt (bringt) von einem Platz an den anderen“, *za dāi* (vgl. Fußnote 15) „überträgt an sich, nimmt an sich“ $\nu\acute{\epsilon}\mu\epsilon\iota\nu$ zu *n'ehmen* etc. Vgl. zuletzt Szémérenyi, *KZ* 74, 1956, 181–184, und Kretschmer's Bericht (*Gl* 19, 207 ff.) über K. Wlaschim's Dissertation *Studien zu den idg. Ausdrücken für Geben und Nehmen* mit vielen anderen Parallelen.

²⁶⁾ Vielleicht heißt $\delta\acute{o}\varsigma \mu\omicron\iota \tau\eta\acute{\nu} \chi\epsilon\acute{\iota}\rho\alpha$ (oben unter 5) ursprünglich – so wie im Englischen – „take my hand“?

²⁷⁾ Vgl. die statistische Übersicht, *Untersuchungen zur Kasussyntax* 103 f.

κουριδίην ἄλοχον καὶ κτήματα πολλά N626 ²⁸⁾, ὑμέων . . . κεφαλαί τε πρόσωπά τε νέρθε τε γούνα υ351—352, ἡμέων κεφαλὰς καὶ νῆϊα δοῦρα ι498 ²⁹⁾. In ἐμεῖο Ἑρῆ ἐκ θυμὸν ἔλγεται Φ112 ist ἐμεῖο ablativisch (adverbal) aufzufassen, vgl. B690, I330, T137 u.ä. Angesichts der häufigen Überlieferungsschwankungen zwischen μευ und μοι (κ481, β262, δ762, ζ239, A37 usw.), bei denen zumindest ein Teil der Genitive auf alte Grammatikertheorie zurückgeht ³⁰⁾, werden wir vielleicht trotz Havers, *Untersuchungen zur Kasussyntax* 96, den ψ230 „schwach bezeugten“ Dativ doch bevorzugen und uns erlauben, an den zwei einzigen Reststellen (σευ φίλα γούναθ' ἰκάνω ν231, μευ πατέρ' ἄμφεπένοντο ο467) den Genitiv des Personalpronomens durch den Dativ zu ersetzen.

Fraglich bleibt noch, ob sich unsere formalsyntaktische Kategorie der Inalienabilienbezeichnungen wirklich von den Havers'schen „Dativus sympatheticus“-Gruppen durch die bei Havers zuzügliche Klasse der Verba des Wegnehmens ³¹⁾ unterscheidet. Laut *Untersuchungen zur Kasussyntax* 88, 99 ist bei diesen Verben bei Homer „auch der pronominale symp. Dativ sehr selten“. Erstaunlich ist, daß hier Havers von der nominalen Kategorisierung (I. Leib des Menschen, II. Seele des Menschen, III. zum Besitz des Menschen gehörige Personen oder Gegenstände, V. Beziehungen der Menschen zueinander) ab- und zu einer Einteilung nach dem regierenden Verbum übergeht. Diese Inkonsistenz erschien ihm notwendig, weil eine Reihe von regierenden Substantiven übrigblieben, die sich nicht in die genannten vier Kategorien zwängen liessen. Havers' Belege für den adnominalen Dativ des Personalpronomens bei den Verben des Wegnehmens sind die folgenden (regierende Substantiva von mir hervorgehoben): μοι γέρας . . . ἀφαιρήσεσθαι A161, γέρας . . . μοι . . . ἔλετο I367, τοι . . . ἐκ θυμὸν ἐλέσθαι A380, μοι . . . ἐκ θυμὸν ἔλοιο υ61, μοι σίνοιτο . . .

²⁸⁾ Havers, *Untersuchungen zur Kasussyntax* 87.

²⁹⁾ *Ibid.* 94.

³⁰⁾ Ludwig, *Aristarchs homerische Textkritik* I. 176, 544, 562.

³¹⁾ Havers hat noch eine Untergruppe der Verben des „Abwehrens“, sagt aber selbst, daß bei dieser Gruppe erstens sehr häufig auch Substantiva im Dativ stehen, zweitens der Dativ mit dem adverbale (ablativischen) und nicht dem adnominalen (possessiven) Genetiv wechselt (*Untersuchungen zur Kasussyntax* 90). Also hat diese Gruppe, wie auch Kategorie VI („Verba der Bewegung, wo der Dativ mit „lokalen Fügungen“ im Wechsel steht), nichts mehr mit den adnominalen Dativen zu tun.

ἐταίρους μ114, τοι . . . ἀφείλετο νόστιμον ἦμαρ τ369 (cf. τοῖσιν ἀφείλετο νόστιμον ἦμαρ α9), πόντος . . . οἱ οὐ τιν' (scil. ἐταῖρον) ἀπηύρα γ192; die den Dativ regierenden Substantiva fallen nicht unter Havers' Sachbegriffskategorien, aber ausnahmslos in die von uns auf Grund von φίλος aufgestellte syntaktische Kategorie. Außer diesen Stellen gibt es nur noch νῆά μοι οἴχετ' ἄγων δ634 und das ähnliche (von Havers übersehene) μή τί μοι οἴχονται κοίλης ἐπὶ νηὸς ἄγοντες ν216, aber vielleicht verbindet sich hier μοι mit οἴχεσθαι („entweichen mir“) und ἄγειν mit νῆα, bzw. τι.

7. So lassen sich also die homerischen Substantiva in zwei formelle Klassen teilen: Kennzeichen der einen Klasse ist die Verbindung mit φίλος, opponierendes Kennzeichen der zweiten Klasse (mit der III.1 gemachten Einschränkung) die Verbindung mit ὁ ohne sonstige adnominale Ergänzung; zweites Kennzeichen der ersten Klasse ist die Verbindung mit adnominalem Dativ der Personalpronomina, das entsprechende der zweiten Klasse — Verbindung mit dem entsprechenden adnominalen Genitiv.

Paradigmatisch:

φίλος ἐταῖρος	„der zu (jemandem) gehörige ³²⁾ Freund“	ὁ ἵππος	„das Pferd“
ἐταῖρός μοι	„der mir (zugehörige) Freund“	ἵππος μευ	„das Pferd von mir“,

womit nicht nur ein im Homerischen zutreffendes Übersetzungsäquivalent für attributives φίλος gefunden, sondern auch die Oppositionen der folgenden Art (vgl. II.2.B) erklärt sind:

φίλον δῶμα	„das (jemandem) zugehörige Haus“	τὸ δῶμα	„das Haus“;
δῶμά μοι	„das mir (zugehörige) Haus“	δῶμά μευ	„das Haus von mir“.

IV

1. In manchen Sprachsystemen, in denen für die Inalienabeln kein eigener Possessionsausdruck vorliegt, sondern die auch sonst gebräuchlichen Possessivpronomina benutzt werden, drückt sich die Sonderstellung der Inalienabilienbezeichnungen durch die begrenzte paradigmatische Substitution des Possessivpronomens aus, d.h. während z.B. in *he has his money* das Pronomen *his* durch *my*, *your*, *her*,

³²⁾ Auch in Blackfoot steht „Kamerad“ in der Inalienabelnklasse (Uhlenbeck, o.c. 359).

our, their paradigmatisch ersetzbar ist, ist dies in *he has his birthday* nicht der Fall. Theoretisch gesehen: im Kategorialparadigma *my, your, ... their* besitzt jedes Glied seine Bedeutung im Kontrast zu den anderen substituierbaren Gliedern (d.h. *his* ist nicht „*my, your, ... their*“, z.B. *his money* $\neq$ *her money* usw.), wo aber keine Substitutionsmöglichkeit vorliegt, ist die Bedeutung nicht abgegrenzt; in *he has his birthday* ergibt sich die Bedeutung von *his* nicht aus der Abgrenzung gegenüber unmöglichen Formen wie '*he has my birthday*' etc., *he* hat also keine Person- oder Possessionsfunktion (da sein Aufscheinen bedingt — obligatorisch — ist, denn an Stelle von *X* in *he has X birthday* kann nur *his* stehen, und da es auch nicht wegbleiben kann, ist es funktionslos, linguistisch ohne Relevanz). Das liegt eben im Wesen der Konstruktionen mit den Inalienabilienbezeichnungen, da die „Person“ des Besitzers durch den Ausdruck der possessiven Beziehung selbst schon definiert ist (Possessionsbezeichnung = Identifikation, s. III.2; Inalienabilität ist nichts anderes als Ausschluß des Personenwechsels, d.h. Besitzerwechsels). Daher ist in dem hier beispielsweise analysierten englischen Ausdruck nicht mehr ausgedrückt als im entsprechenden deutschen *er hat Geburtstag*, wo das funktionslose, irrelevante Element einfach nicht da ist.

2. Diese Überlegung war notwendig, um die Verwendung possessiver Ausdrucksformen in Anreden zu verstehen. In vielen Sprachen ist der Ausdruck der Anrede durch ein festgewachsenes und unsubstituierbares Possessiv der I. Sg. üblich: man braucht nur an *Madame, Mesdames, Mylord, meine Damen und Herren, mein Kind, mein Lieber* etc. zu erinnern³³⁾, d.h. im Wesen besteht hier wieder eine durch andere Personen nicht substituierbare Zugehörigkeitsform, Ausdruck der Zugehörigkeit zur Person des (oder der) Sprechenden. Das passende Pronomen im Homerischen ist also hier wieder φίλος: γέρον φίλε, φίλε Φοῖβος heisst also nicht „lieber Greis, lieber Phoibos“, sondern „mein Alter, mein Phoibos“; ebenso ist θεοὶ φίλοι ω514 keine Invokation der Götter (die rhetorische Frage τίς νύ μοι ἡμέρη ἦδε wird nicht an die Götter gerichtet), sondern etwa „mein Gott!“.

Belege:

γέρον φίλε Ω650, γ357
θεοὶ φίλοι ω514

φίλε ἐκυρέ Γ172
φίλον θάλος X87

³³⁾ Wackernagel, *Vorlesungen über Syntax* I. 310.

νύμφα φίλη	Γ130, 8743	φίλε Φοῖβε	Ο221, Π667
πάππα φίλ'	ζ57	φῖλε κασίγνητε	Δ155, Ε359, Φ308
ξεῖνε φίλ'	α158, τ350	φίλη κεφαλή	Θ281
μαῖα φίλη	υ129, ψ11, ψ35, ψ59, ψ81		
πάτερ φίλε	π222, ψ124, ω511		
παῖδε φίλω	Η279		
τέκνον φίλε	ο125	φίλε τέκνον	Χ84, β363, γ184, ο509
τέκνα φίλα	δ78, γ418	φίλα τέκνα	Κ192
τέκνον φίλον	ψ26	φίλον τέκος	Γ162, Γ192, Ε373, Φ509, Θ39, Χ183, Ι437, Ι444, Ξ190, Χ38, Ω373, 8611, π25, τ474, ψ5

Vor das Substantiv gestelltes φίλος ist nur dann belegt, wenn das regierende Substantiv ein Inalienabile bezeichnet; auch Φοῖβος, von seinem Vater Zeus angeredet, ist inalienabel; keine Abweichung hiervon ist die Konstruktion φίλε τέκνον oder φίλον τέκος, auch wenn nicht das leibliche Kind angeredet wird; die Lexika geben ungenaue Information, wenn sie behaupten, τέκνον (τέκος) könne bei Homer entweder „filius“ oder „infans“ bedeuten, in anderen Worten entweder einen „inalienabeln“ oder einen „alienabeln“ Begriff bezeichnen. Nichtvokativisches τέκνον oder τέκος bezieht sich immer auf Söhne oder Töchter, nur im Vokativ mag jemand als τέκνον, τέκος angeredet sein, ohne daß der Sprechende sein Vater oder seine Mutter ist; das ändert aber nichts an der lexikalischen Bedeutung oder syntaktischen Klassifizierung von τέκνον oder τέκος; der Jüngere wird einfach vom Älteren als „mein Sohn, meine Tochter, mein Kind“ angeredet.

Die nicht in die Inalienabelnklasse fallenden Substantiva (γέρων, θεοί) bekommen φίλος nachgestellt; es mag sich hier also viel eher um eine Apposition handeln: γέρον, φίλε; vgl. φίλος, ὃ Μενέλαε (I.2), was den attributiven Gebrauch des adjektivischen φίλος wieder nur auf die Inalienabilienbezeichnungen beschränkt erscheinen läßt.

Daß in der Konstruktion des vorgestellten Vokativs φίλε mit einem Inalienabelsubstantiv (φῖλε κασίγνητε) ein wahres attributives Possessivverhältnis zum Regens ausgedrückt wird, ergibt sich auch aus der Tatsache, dass in solchen Konstruktionen φίλος durch eine entsprechende Form von ἐμός ersetzbar ist, während sonst ein Vokativ eines Possessivpronomens der I. Person nicht steht. Belegt sind folgende Verbindungen mit Vokativen der Possessivpronomina der I.

Person: παῖδες ἐμοί γ475; ἐμοὶ ἐρίηρες ἐταῖροι ι172, Ψ6; τέκνον ἐμόν α64, ε22, λ155 u.ö.; μῆτερ ἐμή α346, λ164, λ210 u.ö.; ὃ πάτερ ἡμέτερ α45, α81, ω473, Θ31; ἐμοὶ μνηστῆρες β96, ω131. Das letztgenannte Substantiv wurde bisher nicht als in die Inalienabelnklasse gehörig erkannt, da von ihm kein Beleg mit φίλος vorliegt; nirgends aber zeigt es ein syntaktisches Verhalten, das mit den Kriterien der Inalienabelnklasse im Widerspruch stünde, vielmehr steht es τ488, φ213, τ496 χ307 mit adnominalem Dativ eines Personalpronomens. Auf Grund der hier gebrachten Evidenz wollen wir es also in die Klasse der Inalienabilien einreihen, vgl. VII.1 unter 4.

3. In Anbetracht des Obigen ist es klar, daß die substantivierten vokativischen Anredeformen (ὦ)φίλε, φίλος (I.2), (ὦ) φίλοι, (ὦ) φίλη, (ὦ) φίλαι nichts anderes bedeuten als „meine Herren“ etc. oder vokativische „Monsieur, Madame, Messieurs, Mesdames“.

Sie stehen auch meist in der Eröffnung längerer Reden: φίλ' γ103, γ211, δ204, ν228, ξ149, ο260, π91, ρ593, χ367, ω400; φίλε ξ115; φίλος γ375, ρ17, ρ414, (ὦ φίλος Pal., δός, φίλος codd.); φίλοι κ174, κ226, λ344, μ154, μ208, μ320, π346, π400, σ36, σ52, σ414, υ322, φ152, χ70, χ132, χ248, χ262, ω426, Β79, Β110, Ε529, Ε601, Ζ67, Η191, Ι17, Κ204, Κ533, Λ276, Λ587, Μ269, Ο561, Ο661, Ο733, Ρ248, Ρ415, Τ78, Χ378, Ψ457; φίλαι δ722; am Anfang längerer Reden Imperativ, gefolgt von Vokativ von φίλος: δεῦρο, φίλη (v.l. γύναι) θ292; κλύτε (δεῦτε) φίλοι β410, θ133, ξ495, Β56, Ν481, ἀμφὶ μάλα πράττεσθε Σ254, σχέσθε, Χ416; am Anfang kürzerer Reden: φίλοι ι408, υ245, Ρ421. Sonst nur φίλος Κ169, Ι601, Ψ627, Ψ343; φίλοι Β299, Ζ112, Θ174, Λ287, Ο487, Ο734, Π270, Π544, Ρ185, Ψ787; σύ, φίλος Ψ313, α30, γ199, γ313, θ413; zu φίλος ὦ Μενέλαε vgl. I. 2. Morphologisch ist hervorzuheben, daß sich substantivisches φίλε nur in der Odyssee findet.

V

1. Eine Folge der Ausschaltung des adnominalen possessiven Genitivs der Personalpronomina von der Verbindung mit den Inalienabilienbezeichnungen ist die dadurch — zumindest auf dem Gebiete der Personalpronomina — eintretende Gleichheit des Zugehörigkeitsausdrucks in attributiver und prädikativer Stellung.

Wie in vielen Sprachen sind wir es in unserer elementargrammatischen Auffassung auch vom Griechischen her gewöhnt, daß sich in einer Transformation wie

(attributives Syntagma) (prädikatives Syntagma)
(ὁ) ἀγαθὸς δοῦλος ↔ (ὁ) δοῦλος ἀγαθὸς ἐστι

das Adjektiv nicht einfach durch ein Possessivattribut ersetzen läßt,

sondern daß im Allgemeinen im attributiven Syntagma ein Genitiv, im prädikativen hingegen ein Dativ zu stehen kommt:

(ὁ)(τοῦ) ἀνδρὸς δοῦλος ↔ (ὁ) δοῦλος (τῷ) ἀνδρί ἐστι,
oder (ὁ) δοῦλός μου ↔ (ὁ) δοῦλός μοι ἐστί.

Bei den homerischen Inalienabilienbezeichnungen ist dies nun nicht so, sondern

μοι ἑταῖρος ↔ ἑταῖρός μοι ἐστί,

was der syntaktischen Situation in manchen außerindogermanischen Sprachen (z.B. türkisch, ungarisch) entspricht, in denen (wenn A ein das Besitzobjekt, B ein den Besitzer bezeichnendes Substantiv und P ein Personalmorphem ist)

a. A + P (z.B. III.sg.m.) „sein A“ ↔ A + P + Verbum existentiae
„er hat A“

oder

b. A + P + B „B's A“ ↔ B + A + P + Kopula „B hat A“;

Die Formel (a) trifft auch auf die homerischen Inalienabilien zu:
P(οἰ) + A „sein (inalienables) A“ ↔ P(οἰ) + A + ἐστί „er hat (ein) A“.

2. Eine derartige Entsprechung ist scheinbar auch die syntaktische Grundlage des prädikativen Gebrauches des adjektivischen φίλος (I.2). Prädikativ steht adjektivisches φίλος nicht bei Inalienabeln, also nur bei Sachbezeichnungen, bei denen die Besitzerbeziehung nicht schon gegeben ist. Ein Ausdruck wie ἑταῖρος (μοι) φίλος ἐστί' wäre im Rahmen des Homerischen pleonastisch-tautologisch, jedoch (ὁ) ἵππος φίλος ἐστί' auch unmöglich, ist doch in diesem Falle nur gesagt, dass „das Pferd eigen“ oder „zugehörig ist“, ohne die unbedingt notwendige und das eigentliche Prädikat darstellende Tatsache mitzuteilen, wem eigen oder gehörig. Diese Ergänzung erfolgt aber bei φίλος regelmäßig — von Blutsverwandtenbezeichnungen mit pleonastischem Genitiv (III.3) abgesehen — mit dem Dativ, also (ὁ) ἵππος μοι φίλος ἐστί. Diese Konstruktion ist wohl dadurch gestützt, daß bei prädikativen Besitzerangaben überhaupt der Dativ Verwendung findet.

Wesentlich ist festzustellen, daß die sich hier ergebenden syntaktischen Einschränkungen φίλος sich wieder vollkommen in den Rahmen der Possessivpronomina stellen. Wie der Konstruktion φίλος ἑταῖρος

nicht ,ἵππος φίλος ἐστί', sondern nur ἵππος μοι φίλος ἐστί gegenübersteht, finden wir bei Homer auch gegenüber (ὁ) ἐμός ἵππος nicht ,ὁ)ἵππος ἐμός ἐστί', sondern nur ὁ ἵππος ἐστί μοι. Sehr charakteristisch ist es, daß die Zulassung des nicht dativergänzten φίλος als substantivisches (d.h. nicht genuskongruentes, s. I.2) Prädikatsnomen ihre genaue Entsprechung in σός εἰμι „ich bin der Deinige" (nicht „dein"!) ι529 hat. Die Syntax von φίλος ist also bis hierher die der Possessivpronomina. Besteht andererseits die III.2 aufgestellte Äquivalenz des φίλος mit einem determinativen Element, so heisst τι φίλον μοί ἐστι „etwas ist das Meine, meine Sache, meine Eigenheit", τις φίλος μοί ἐστι „jemand ist einer der Meinigen", etc.

Belege:

1. Νῦν σφῶν ἀμύνεσθαι φίλον ἔστω „eure Sache, vestra res, vestri esto" Π556, μοι αἰεὶ νῆες... φίλαι ἦσαν καὶ πόλεμοι ξ224, αἰεὶ γάρ τοι ἔρις... φίλη A177, E891, ἐμοὶ φίλ' ἀληθέα μυθήσασθαι „meine Eigenheit, mei proprium" ρ15, αἰεὶ τοι μῦθοι φίλοι ἄκριτοί εἰσιν B796, vgl. ν295, αἰεὶ τοι τὰ κάκ' ἐστί φίλα φρεσὶ μαρτυρεῖσθαι A107, οὐ τοὶ ἀποσχέσθαι φίλον ἦεν ι211, σοὶ... οὐ πῶ φίλον ἐστί δαήμεναι οὐδὲ πυθέσθαι ν335, ἐμοὶ οὐ φίλον ἐστί μεταλλῆσαι καὶ ἐρέσθαι ξ378, μοι φίλον ἔσκε μεταλλῆσαι καὶ ἐρέσθαι ο362, ἔργον... μοι οὐ φίλον ἔσκεν ξ222, αἰεὶ... ἡμῖν δαίς τε φίλη κίθαρίς τε χοροὶ τε θ248, ἐμοὶ τὰ φίλ' ἔσκε „meine Angelegenheit war immer" τὰ που θεὸς ἐν φρεσὶ θῆκεν ξ227, οὐ... Τυδεΐ... φίλον πτωσκαζέμεν ἦεν Δ372. Die φίλος auf Grund seiner Äquivalenz mit αὐτός begleitende Kontrastivbedeutung (III.3) kommt in einer Reihe der Kontexte in den obigen Stellen zum Ausdruck, d.h. es soll gesagt werden: „im Gegensatz zu anderen, zu mir etc. ist es deine, meine Eigenheit" u.ä.

2. Μὴ τοῦτο φίλον („selon lui, nach seinem Sinn") Διὶ πατρὶ γένοιτο η316, φίλον ἔπλετο θυμῷ H31, πέρι κῆρι φίλος γένετ' ἀθανάτοισιν Ω61, σφι φίλος περὶ κῆρι Ω423, vgl. κ66, τότε πᾶσι (ὑμῖν) φίλον καὶ ἡδὺ γένοιτο Δ17, H387 θεοῖσι φίλος τοσσόνδε γένοιτο X41, σὺ μὲν οὐκέτ' ἐμοὶ φίλα „selon moi, in meinem Sinne" ταῦτ' ἀγορεύεις, οἶσθα καὶ ἄλλον μῦθον ἀμείνονα τοῦδε νοῆσαι H357, M231, Σ285.

3. Φαιήκεσσι φίλος „einer der Ihrigen" πάντεσσι γένοιτο θ21, zu σὲ Φαιήκεσσι φίλον πάντεσσιν ἔθηκα ν302 vgl. Fußnote 5, Διὶ... φίλον „einer der Seinigen" ἔμμεναι ω25, φίλος ἦσθα θεοῖσιν ω92, φίλος Διὶ πατρὶ γένοιτο ξ440, ο341, πολλοῖσιν Ὀδυσσεὺς ἔσκε φίλος τ239-240, ἦ ῥα... Αἰνείας φίλος ἀθανάτοισι θεοῖσιν Υ347, μάλα γὰρ φίλοι ἀθανάτοισιν ζ203, μάλα οἱ φίλος ἦεν A381, εἴ τοι φίλος „einer der Deinigen" ἐστί Π450, πᾶσι φίλος καὶ τίμιός ἐστιν ἀνθρώποις κ38, ζῶός περ ἐὼν φίλος ἦσθα θεοῖσιν „gehörtest du zu den Göttern"(!) Ω749, φίλος... ἦν ἀνθρώποις Z14 εἴ τις τοι καὶ κεῖθι φίλος „wenn du auch dort jemanden hast" μερόπων ἀνθρώπων Γ402.

4. Σφι φίλη „angetraut, Angehörige" (vgl. VI) καὶ αἰδοίη Ε210, vgl. Σ386, Σ425 (I.2).

3. Es wird nicht wundernehmen, daß Appositionen und N.pl.-

Formen das syntaktische Verhalten des adjektivischen prädikativen φίλος zeigen. In der folgenden Übersicht sind die Gruppennummern so eingeführt, daß sie denjenigen in Paragraph 1 entsprechen; naturgemäß entsprechen Gruppen 1 und 2 den Ntr. pl., 3 den animierten appositionellen Formen:

1. Δμῶς ἀναγκαῖοι, τοῖ οἱ φίλα „seine Angelegenheiten, eius res“ ἐργάζοντο ω210; Ἀτρεΐδης καὶ ἐγώ, φίλα εἰδότες ἀλλήλοισιν „knowing the affairs of each other“ γ277; ἐμοὶ γε ἐνθάδε ναιετάων φίλος „einer der Meinigen“ εἴη καὶ φίλα „res meas, meine Interessen“ ἔρδοι ο360.

2. In der Konstruktion τινι φίλα φρονεῖν „an jemandes Angelegenheiten denken, an jemanden denken, curare alicui“ im Partizip: φίλα φρονέων... ὥς τε πατήρ ᾧ παιδί α307, φίλα φρονέουσ' Ὀδυσῆι η15, πατρὶ φίλα φρονέων (-έουσα) Δ219, E116, οἱ... φίλα φρονέουσ' ἐνὶ θύμῳ η42, ebenso π17, ζ313=η75.

3. a. Ὀμίχλην ποιμέσιν οὔτι φίλην „nichts für die Hirten“ Γ11.

b. Αἰολὸς Ἰπποτάδης, φίλος ἀθανάτοισι θεοῖσι κ2.

c. In Διὶ φίλος „zu Zeus gehörig, Mann des Zeus, ὁ τοῦ Διός“ äquivalent mit διογενής: Ἀπόλλωνα Διὶ φίλον A86; ᾧ Ἀχιλεῦ... Διὶ φίλε „Mann des Zeus“ (διογενής auf Achilles Φ17) A74, ebenso X216, Π168, Ω472; Πάτροκλε Διὶ φίλε (διογενής auf Patroklos A337 u.ö.), Ὀδυσῆα Διὶ φίλον (διογενής auf Odysseus K340 u.ö.) Λ419, Λ473, K527; Ἐκτωρ... Διὶ φίλος Z318, Θ493, K49, N674; Φοῖνιξ... Διὶ φίλος I168; κήρυκες... Διὶ φίλοι Θ517, vgl. θεῖον γένος οἱ κήρυκες Eust. 1397₅₆ etc., vgl. κήρυκες θεῶν Γ245.

d. In Ἀρηϊ φίλος „zum Ares gehörig, Ἀρήϊος“: Ἀρηϊ φίλον Μενέλαον Γ52 u.ö. (ἀρήϊος auf Menelaos Γ339 u.ö.), Ἀρηϊ φίλος Λυκομήδης P346, Ἀρηϊ φίλος Μελέαγρος I550, ἀρχὸν Ἀρηϊ φίλον B778; nicht mehr in rein appositioneller Stellung, also eher attributiv und an der Grenze zum Kompositum ἀρηϊφίλος³⁴) in der stehenden Phrase ἀρηϊφίλων ὑπ' Ἀχαιῶν Z73, Π303, P319, P336; die gesperrte Stellung zeigt jedoch, daß volle Adjektivierung noch nicht erfolgt ist, also etwa ἀρηϊφίλων ὑπό „von den Kriegerischen“, Ἀχαιῶν „den Achaeern“.

VI

1. Wir können uns nunmehr den substantivierten Formen φίλος m., φίλη f., zuwenden. Prädikativ stehen sie im Sinne von „(als) einer der Unrigen, Seinigen etc.“ (Belegliste unter I.2, 4c). Als Bezeichnung für eine „zugehörige Person“ (III.7) entspricht eine solche Form ziemlich genau dem deutschen „Angehörige(r)“; so schon Eustathius 398₄₅. (ad Γ163): πηοὺς μὲν δοκεῖ λέγειν τοὺς ἐξ ἀρχιστείας οἰκείους,

³⁴) Bei Ἀρηϊ φίλος steht morphologisch nichts im Wege, es als Kompositum aufzufassen, da – im Gegensatz zu Διὶ im Sinne von „Zeus“ – Ἀρηϊ- auch als Vorderglied existiert. Warum aber dieser Auffassung hier nicht Raum gegeben wird, ersehe man aus VII. 2. Allerdings wird Ἀρηϊ φίλος im Gegensatz zu Διὶ φίλος dem Namen meist vorangestellt.

φίλους δὲ τοὺς ἀφ' αἵματος (vgl. das Zitat unter II.1); auch der Scholiast zu α238: τοὺς ἐταῖρους ἀπὸ τῶν φίλων διαιρεῖ ὁ ποιητής. οἱ μὲν γὰρ ἐταῖροι οἱ προσοικειωθέντες κατὰ φιλίαν, οἱ δὲ φίλοι οἱ οἰκεῖοι καὶ κατὰ γένος προσήκοντες.

Hierher gehören vorerst das von Eustathius wie zitiert kommentierte ὄφρα ἔδη πρότερόν τε πόσιν πηοὺς τε φίλους τε „die Angehörigen“ Γ163, μή μιν πρὶν ἄλλος γνοίη ἀστοί τε φίλοι τε ν192, οὐ γάρ πω ἴσασι φίλοι κατὰ δῶμαθ' ἐκάστου ω188, φίλους „seine Angehörigen, die Seinen“ τ' ἰδέειν καὶ ἰκέσθαι δ475, ε41, ε114, ζ314, η76, δς (Odysseus) . . . φίλων „seine Angehörigen, die Seinen“ ἀπο πῆματα πάσχει νήσῳ ἐν ἀμφιρύτῃ α49, vgl. η152, θ411, τῆλε φίλων ἀπόληται ἀλώμενος β333, vgl. Π539, ε113, Λ817, ἀνευθεν ἄγων πατρός τε φίλων τε Φ78. Ferner im erweiterten Bedeutungsfeld „die Seinen“³⁵): φίλων ἐν χερσίν, ἐπεὶ πόλεμον τολύπευσε α238, δ490, ξ368, φίλων καὶ πατρίδος αἵης δηρὸν ἀπέσσεσθαι σ145, τ301, vgl. ω290, πόντον ἐπ' ἰχθυόεντα φίλων ἀπάνευθε φέρουσιν T378.

Charakteristisch ist, daß diese Gruppe — formal identifiziert als substantivische nicht prädikative oder attributive männliche und weibliche Formen — auf den Plural beschränkt ist (außerdem haben die hierhergehörigen substantivierten φίλος-Formen nie beschreibende Attribute, s.u.). Substantiviertes nichtprädikatives singularisches φίλος ist also bei Homer ebenso wenig zu erwarten wie etwa ein Singular von *nostri* oder von *die Seinen* u.ä. Daß man eine solche Form (im Plural) eventuell an manchen Stellen auch „seine Lieben“ u.ä. übersetzen kann, tut nichts zur Sache, denn das in diesem Ausdruck wesentlich φίλος Entsprechende ist nicht „Lieben“, sondern das Possessiv („seine“ etc.), denn „die Lieben“ läßt sich wohl in solchen Kontexten nicht gebrauchen.

Das substantivierte nichtprädikative φίλος kann nur zwei Arten von Modifikationen bei sich haben: a. πάντες, b. reflexives δς, wobei die Beschränkung auf den Plural erhalten bleibt: a. φίλοι ἅμα πάντες ἔποντο Ω327, ἐν δ' ὅμῳ ἐρέω πάντεσσι φίλοισι I528; b. δὴν ἀπάνευθε φίλων ὧν ἔσσεται β164, θυμὸν ἐτέρπετο οἷσι φίλοις Φ45, vgl. α19. Bemerkenswert ist, dass trotz der adjektivischen Natur dieser beiden Modifikationen keine deskriptiv ist. Φίλος kann also nicht „beschrie-

³⁵) „Wenn Verwandte und Freunde φίλοι genannt werden, so werden sie dadurch als ‚die eigenen‘ bezeichnet“ (Brugge, KZ 20, 1872, 42). Blackfoot „ ‚mijn verwanten‘, ‚mijn gezellen‘ . . . beteekent . . . etymologisch niet anders dan ‚de personen die bij mij behooren‘ ” (Uhlenbeck, o.c. 355).

ben" (vgl. III.2) oder „restringiert“ werden; φίλος ist in der Ilias und Odyssee noch nicht zu einem vollen Substantiv geworden, denn es läßt keine restriktiven Attribute (auch keinen attributiven Genitiv) zu. Ebenso wenig verläßt es irgendwo das semantisch-syntaktische Feld eines Pronomens zum Ausdruck der durch untrennbare Zugehörigkeit erfolgten Identifizierung. Kurz: es heißt noch nicht „lieb“. Diese Funktion scheint ihm auf Grund der durch prädikatives und substantiviertes φίλος ermöglichten Begriffsverschiebung (φίλος . . . ἦν ἀνθρώποισι, πάντας γὰρ φιλέσκειν Z14!) erst infolge an seinen Ableitungen erfolgten Bedeutungserweiterungen und hauptsächlich durch seine volle Substantivierung in späteren Sprachschichten zugewachsen zu sein.

2. Läßt sich nun auch das obskure φίλως in den Rahmen der φίλος-Formen einordnen? Je nach der Person des modifizierten Verbums sollte es etwa „auf meine, deine . . . eigene Art; nach meiner, deiner . . . Eigenart“ bedeuten (vgl. zu φιλεῖν, VII.2); also eventuell: σφῶν μὲν τ' ἐπέοικε „zwar ist es sonst eure Art“ μετὰ πρῶτοισιν ἐόντας ἐστάμεν, kontrastiert durch νῦν δε φίλως χ' ὁρώμετε καὶ εἰ δέκα πύργοι Ἀχαιῶν ὑμείων προπάροιθε μαχοίατο „jetzt aber würdet ihr nach eurer Eigenheit wohl zusehen . . .“ Δ341—347; καρπαλίμως χαίροντα φίλως χαίροντες „sich auf ihre (eigene) Art freuend; sich ihrerseits freuend“ ἐπεμπον εἰς Ἰθάκην τ461 (es liegen aber auch Lesarten ohne φίλως vor). Vielleicht läßt sich dieses Wort also jetzt eher verstehen als bisher.

VII

1. Die Belege von φιλίων, φίλτερος, φίλτατος lassen sich syntaktisch ebenso gruppieren wie die der Grundform:

1. Attributiv bei Inalienabelnbezeichnungen: οἱ πόλυ φίλτατος ὤλεθ' ἐταῖρος P411, P655, οἱ . . . φίλτατοι ἄνδρες „meine Leute“ ἐμῷ ὑπέασι μελάθρῳ I204;

2. Prädikativ:

a. mit dativischer Ergänzung: ἐμὶ τρεῖς . . . πολὺ φίλταταί εἰσι πόληες Δ51, οἷ (scil. φίλοι ἄνδρες) μοι . . . Ἀχαιῶν φίλτατοί ἐστων I198, οἷ (scil. ἄνδρες) . . . σοι αὐτῷ φίλτατοι Ἀργείων I522, ἐταῖροι οἷ οἱ . . . φίλτατοι ἦσαν ἀπάντων I586, σοι . . . φίλτατόν ἐστιν ἀνδρὶ ἐταιρίσσαι Ω334, Αἰνείαν ὃς ἐμοὶ πάντων πολὺ φίλτατός ἐστιν Ε378, πέπλον ὃς οἱ δοκέει . . . εἶναι . . . πολὺ φίλτατος αὐτῇ Ζ90—91, Ζ272, Ἡρακλῆος . . . ὃς περ φίλτατος ἔσκε Διὶ Κρονίωνι ἀνακτι Σ118, οἱ μετὰ παισὶ νεώτατος ἔσκε γόνιοι καὶ οἱ φίλτατος ἔσκε Υ409—410, μοι . . . πολὺ φίλτατος ἦσθα γνωτῶν

X233, "Εκτωρ φίλτατος ἔσκε θεοῖσι βροτῶν Ω67, τί μοι πεφιδέσθαι ἐνὶ φρεσὶ φίλτερον ἦεν Τρώων Φ101, ὃς σεῦ ἅμα κρείσσων καὶ φίλτερος ἀθανάτοισιν Υ334, νῦν . . . ἐγγύθι μοι θάνατος κακός, οὐδ' ἔτ' ἀνευθεῖν, οὐδ' ἄλέη. . . τό γε φίλτερον ἦεν Ζηνί X301—302, ἥ οἱ γαίῳ πολὺ φίλτατῇ ἐστὶν ἀπάσέων Θ284, ὅς τέ οἱ (σοὶ) αὐτῷ φίλτατος οἰωνῶν Ω293, Ω311, μέμαμεν . . . τοι ἔξοχον ἄλλων κήδιστοί τ' ἔμεναι καὶ φίλτατοι ἔσσοι Ἀχαιοί I641—642, αἰδοιότερος καὶ φίλτερος ἀνδράσιν εἴην λ360.

b. Οὐ . . . πῶ τις . . . φίλιον ἐμὸν ἔκετο δῶμα τ351, ω268.

3. Appositionell mit dativischer Ergänzung: οἱ ἐπὶ γαίῃ κείατο, γύπεσσιν πολὺ φίλτεροι ἢ ἀλόχοισιν Λ162.

4. Die im Konstruktionstypus φίλτατος ἀνδρῶν — der vornehmlich den ungebräuchlichen Vokativ 'φίλτατε ἄνερ' ersetzt — erscheinenden partitiven Genitive gehören, soweit der Superlativ nicht prädikativ steht (s. 2a) ausschließlich der Inalienabelnklasse an: φίλταθ' ἑταίρων ω517, N249, Τ315, φίλτατε παίδων Ω748, φίλτατος (-ον) ἀνδρῶν Π433, O111, ξείνων φίλτατος P584, wozu nur noch δαέρων πολὺ φίλτατε πάντων Ω762 zu stellen ist; da das nur sechsmal (nur in der Ilias) vorkommende Substantiv δαήρ nirgends der Syntax der Inalienabelnbezeichnungen widersprechende Verbindungen eingeht, können wir es auf Grund der hier gebrachten Evidenz (so wie μνηστήρ, IV.2) in die Inalienabelnklasse einreihen.

5. Substantiviertes φίλτερος im Singular: μέλλει μὲν πού τις καὶ φίλτερον ἄλλον ὀλέσσαι, ἦε κασίγνητον ὁμογάστριον ἦε καὶ υἷόν Ω46.

Zu besprechen ist nunmehr das Bedeutungs- und Funktionsverhältnis zwischen φίλιων, φίλτερος, φίλτατος und dem „Positiv“ φίλος, ein Verhältnis, das bei einem Pronomen nicht so sehr auf der Hand liegt wie bei einem Adjektiv, das „lieb“ bedeutet. Relativ einfach liegt die Sache noch bei den sechs Stellen von φίλτερος. Eine Paarung wie φίλος: φίλτερος stellt sich bei einem Possessivadjektiv als der homerischen Morphologie absolut konform dar und läßt sich in die Serie ἄμός: ἡμέτερος, ὑμός: ὑμέτερος, σφός: σφέτερος ohne jegliche Schwierigkeit einordnen — damit ist auch das für φίλ-τερος, φίλ-τατος als „Komparations“ formen bestehende morphologische Problem aus der Welt geschafft. Für die suffigierten Glieder dieser Paare hat Benveniste ³⁶⁾ mit Recht und auf Grund überzeugender Beweisführung die differentiative Grundfunktion „der, welcher (im Gegensatz zu einem anderen) unser etc. (und nicht euer etc.) ist“ in Anspruch genommen. Dasselbe liegt noch in einem Teil der Belege von φίλτερος

³⁶⁾ *Noms d'agent et noms d'action* 116.

vor: ὅς σευ ἅμα κρείσσων καὶ φίλτερος ἀθανάτοισιν „welcher sowohl stärker ist als du und der — im Gegensatz zu dir — ein Mann der Götter ist” Υ334; νῦν δὲ δὴ ἐγγύθι μοι θάνατος κακὸς οὐδ’ ἔτ’ ἀνευθεν οὐδ’ ἄλῃ. ἥ γάρ ῥα πάλαι τό γε φίλτερον ἦεν Ζηνὶ τε καὶ Διὸς υἱὶ ἐκηβόλω X300—302, von den kontrastiv gepaarten Gliedern ist ἄλῃν μοι ἐγγύθι εἶναι dasjenige, welches Ζηνὶ φίλον ist und nicht θάνατον κακόν μοι ἐγγύθι εἶναι; ähnlich wohl μέλλει μὲν πού τις καὶ φίλτερον ἄλλον ὀλέσσαι ἢ ἐ κασίγνητον ὁμογάστριον ἢ ἐ καὶ υἱόν „einer der φίλος, ein Angehöriger, und nicht wie Patroklos, nur ein ἐταῖρος ist” Ω46. Nicht mehr aber trifft dies auf Λ162 zu, wo γύπεσσιν πολὺ φίλτεροι ἢ ἀλόχοισιν schon die typische „Komparativ”konstruktion zeigt, wie auch in τί μοι πεφιδέσθαι ἐνὶ φρεσὶ φίλτερον ἦεν Τρώων Φ101 und in καὶ κεν πολὺ κέρδιον εἶη, πλειοτέρῃ σὺν χειρὶ φίλην ἐς πατρίδ’ ἰκέσθαι. καὶ κ’ αἰδιόιτερος καὶ φίλτερος ἀνδράσιν εἶην λ358—360.

Hier ist φίλος schon auf dem Wege zum steigerungsfähigen Qualitätsbegriff („Zugehörigkeit” nicht als Beziehung, sondern als Eigenschaft), was besonders im prädikativen Superlativ, bei dessen Subjekt im Gegenstaz zum Positiv keine Unterscheidung zwischen Inalienabilien und anderen Substantiven mehr zu treffen ist, zum Ausdruck kommt und mehr noch im isolierten Äquativ³⁷⁾ φιλίων, der seinem Wesen nach Qualitätsbegriff sein muß: οὐπῶ τις . . . φιλίων ἐμὸν ἔκετο δῶμα „niemand kam mir so gelegen, so lieb . . .” τ351, ω268.

Wertvoll mag noch die Feststellung sein, daß die homerischen Komposita mit Vorderglied φιλο-, die sich morphologisch nur als Behuwhis — also Possessivkomposita³⁸⁾ — auffassen lassen, z.B. φιλοπτόλεμος „ᾧ πτόλεμός ἐστι φίλος”, des Wesens ihrer Konstruktion wegen nicht aus dem Rahmen der possessiven Bezugsfunktion von φίλος fallen.

2. Wie allgemein angenommen wird, ist im Denominativ φιλεῖν der Bedeutungsumschwung von „als einen der Seinen betrachten”³⁹⁾ zu „liebhaben” (vgl. φίλος ἦν ἀνθρώποισι, πάντας γὰρ φιλέεσκεν ὁδῶ ἐπὶ οἰκία ναίων Z14—15) oder von „zu der Seinen machen”⁴⁰⁾ oder „besitzen” zur sexuellen Liebe eingetreten, was auch für φιλότης „Zugehörigkeit” > 1. „Gebundenheit (an jemanden)”, 2. „Hingabe

³⁷⁾ Benveniste, o.c. 135.

³⁸⁾ Vgl. Risch, *Homerische Wortbildung* 175.

³⁹⁾ Bugge, l.c.

⁴⁰⁾ Van Leeuwen, *Enchiridium dictionis epicae* 209.

im sexuellen Sinne" gilt. Auch nachhomerisches φιλεῖν mit Infinitiv läßt sich auf pronominales φίλος direkt und nicht erst durch Vermittlung des denominativen φιλεῖν „gern tun" zurückführen, wenn man es auf τινι φίλον ἐστί „es ist jemandes Eigenheit" (V.2, unter 1) zurückführt; φιλεῖν „lieben" ist demnach aktiv, φιλεῖν „pflegen" passiv.

Der Übergang zum genusindifferenten φίλος („Freund" = „Liebender, Geliebter") wie auch zu seiner Verbindung mit einem Genitivattribut ist demnach sekundär und zweifellos nachhomerisch; wahrscheinlich ist dieser Übergang nur auf Vermittlung des denominativen Verbums zurückzuführen. Daß das nicht schon homerisch ist, beweist unter anderem auch das Inventar der mit (-)φιλο- gebildeten homerischen Komposita, die den Vorderglied-φιλο-Typus noch nicht kennen und überhaupt φιλο-nur im Vorderglied besitzen (Διὶ φίλος, Ἄρηϊ φίλος, s. V.3, werden auch durch Zusammenschreibung nicht zu Komposita, da dem φίλος eine von ihm regierte Kasusform vorausgeht, was schon Plato, *Cratylus* 399b, wußte). Der πονηρό-φιλος-Typus konnte nur in einem Sprachsystem entstehen ⁴¹⁾, das bei substantivischem φίλος ein Genitivattribut zuließ (πονηρόφιλος „τοῦ πονηροῦ φίλος") oder (als Rektionskompositum) φίλος als aktives Nomen agentis besaß: beides ist bei Homer nicht der Fall.

VIII

Was hier gezeigt wurde, ist, daß die sogenannte Bedeutungs-entwicklung der Basis φιλ(ε/ο)- eher eine syntaktisch-morphologische Ausbreitungsentwicklung darstellt, deren Ausgangsstufe wir durch

⁴¹⁾ Die Bildung mit Hinterglied -φιλο- „liebend" ist von Aristoteles an belegt: φανηρόφιλος ist das Gegenteil von φανηρομισής (EN 1124b 27), ferner sind aristotelisch φιλόφιλος, πονηρόφιλος, χρηστόφιλος. Eine frühere isolierte Bildung, pin-darisches πολύφιλος, gehört nicht hierher, sondern in die Klasse der Bahuwrihis mit numeralem Vorderglied („viele Freunde habend"). Aus Sappho zitiert man Γελλῶ παιδοφιλωτέρᾳ (fr. 178, Lobel-Page), jedoch Einsichtnahme in die Quellen lehrt, daß nur der Name, nicht aber das Epithet dieses sprichwörtlichen Gespenstes von Zenobius III. 3 aus Sappho zitiert wird. Das Inventar von φίλος bei Sappho lehrt andererseits, dass bei ihr φίλος syntaktisch noch dort steht, wo es bei Homer gestanden hat: πατ[ή]ρ φίλος fr. 44, ἔων φίλος ἄμμι λέχος ἄρυσσο fr. 121, ὦ φίλε γάμβρε fr. 115, φίλων το[κ]ήων fr. 16, φίλοισι Φοῖσι fr. 5, φάμα δ' ἦλθε κατὰ πτόλιν . . . φίλοις fr. 44, ὦ φίλαι fr. 43, φίλαι . . . ἔταιραι fr. 142.

synchronische Bearbeitung des homerischen Sprachmaterials ziemlich genau abzugrenzen imstande sind, und daß diese Abgrenzung gleichzeitig die formale Definition der syntaktischen Kategorie des unveräußerlichen Besitzes im homerischen Griechisch liefert.

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ORIGIN AND CONTROL OF MEANING HYPOTHESES

Summary of contents

The concepts of 'complete meaning hypothesis' and 'origin of a hypothesis' are introduced and some elementary truths of lexicographical methodology are stated by means of them.

1. INTRODUCTION

It is well-known that lexicographers all over the world are engaged in producing many excellent dictionaries. But whereas the amount of energy spent upon the very production of dictionaries is impressive, the literature concerned with methodological aspects of lexicography is scanty and often superficial.

In this paper I attempt an approach to lexicographical methodology which is still rather rare. On the basis of a study of general methodology I have chosen some concepts which, in my opinion, are especially suited for the purpose of stating the most important truths of lexicographical methodology. In that way assumptions which are accepted implicitly or unconsciously by lexicographers, can be formulated explicitly.

As to the results of such an approach I have only modest expectations. Generally, what can be expected is a clear presentation of what was already known, but obscurely. Further, the borderline between the known and the unknown in methodological aspects can be more clearly drawn. Some assumptions, which were formerly believed to be generally accepted, turn perhaps out to be disputed. Finally, on the way some more or less important truths can be detected, depending upon the acumen of the research-worker.

I do not expect many to feel instructed after the reading of the *present* paper. That, however, is a result of the mediocrity of its author. The lack of instruction is not due to the unfruitfulness of the method of approach.

Section 2—5 are largely, but not exclusively devoted to the introduction of terminology. Section 6 and 7 give the main contentions.

2. DEFINITIONS AND HYPOTHESES

Most of the sentences which are produced with scientific purposes in mind fall into one of two classes. The first class comprises sentences expressing *definitions*, the second those expressing *hypotheses*.

An attempt to offer satisfactory sets of necessary and sufficient conditions which may enable one to conclude whether a given sentence expresses a definition or a hypothesis, meets with grave difficulties. Here I limit myself to the following rough characterization.

A decision or proposal to the effect that a form "a" shall mean the same as another form "b" will be called a *definition*. I stipulate that definitions do not have the property of being false or true.

A sentence expressing an assumption, whether empirical or not, whether tested or not, will be said to express a *hypothesis*. It is generally stipulated or known that hypotheses may have the property of being false or true.

Example of a sentence expressing a definition: We propose to use the term "phoneme" with the same meaning as "minimum distinctive sound unit" in linguistic treatises. As this sentence expresses a definition it is not true or false.

Example of a sentence expressing a hypothesis: The term "phoneme" as used by Bloomfield in his book *Language* has the same meaning as "minimum distinctive sound unit". This last sentence expresses an assumption which may be true or false. It is true if the term "phoneme" as used by Bloomfield in his book *Language* has the same meaning as "minimum distinctive sound unit".

3. MEANING HYPOTHESES

The second example which was given in section 2 is not only an example of a hypothesis, but also an example of a meaning hypothesis. A meaning hypothesis is an assumption stating the identity or likeness of meaning of two forms. I stipulate that a sentence expressing a *complete meaning hypothesis* comprises at least four forms. These four forms are the following.

- a) The definiendum. This is the form the meaning of which is described. In the example the definiendum is "phoneme".
- b) The definiens. This is the form describing the meaning of the

definiendum. In the example the definiens is "minimum distinctive sound unit".

c) The meaning relational phrase. This phrase states which relation holds between the definiendum and definiens. In the example it is the phrase "has the same meaning as".

d) The form indicating the field of validity. This form delimits a domain consisting of texts or speech situations, and information which may be found in this domain is relevant for the purpose of testing the meaning hypothesis. In the second example of section 2 the field of validity is denoted by the phrase "as used by Bloomfield in his book *Language*". Occurrences of "phoneme" in *Language* by Bloomfield will be relevant for the truth of the meaning hypothesis expressed by the second example.

If a meaning hypothesis is expressed by a sentence which comprises the 4 above mentioned forms, it will be said to be a *complete meaning hypothesis*. If a meaning hypothesis is expressed by a sentence which does not comprise the 4 above mentioned forms but less, it will be said to be a *rudimentary meaning hypothesis*.

The presence of the above 4 forms is necessary for expressing a complete meaning hypothesis. But a sentence expressing a complete meaning hypothesis may comprise more than the 4 stipulated forms. In the example we find the phrase "the term" in addition to the 4 stipulated forms.

Such additional forms may be practically superfluous, e.g. "the term", but they may also play an important function in delimiting the exact meaning of a meaning hypothesis. In that way additional forms may play an important rôle in adding a delimitation to the conditions of control of the hypothesis.

4. MEANING HYPOTHESES IN DICTIONARIES

If we consider the dictionary articles of monolingual dictionaries without taking into account information given by lexicographers outside the articles themselves, we are justified in asserting that the dictionary articles only contain rudimentary meaning hypotheses as at least the meaning relational phrase is lacking.

Example. In H. C. Wyld: *The Universal Dictionary of the English Language*, London 1936, in the article on "beleaguer" we find the

definiens "to besiege (lit and fig.)". The definiendum is "beleaguer". The meaning relational phrase is lacking, and likewise the form indicating the field of validity.

In other parts of lexicographical literature, however, e.g. the prefaces of dictionaries and literature concerned with lexicographical methodology ¹⁾, we find additional information. This information indicates that it is the intention of lexicographers to assert complete meaning hypotheses. But lexicographers do not agree upon one single meaning relational phrase for each language, and usually not even for one dictionary (cp. e.g. Wyld's dictionary).

If it is so that an important part of the activity of lexicographers consists in establishing meaning hypotheses, then the notion of meaning hypothesis is relevant to the understanding of lexicographical activity. Although it may be the intention of lexicographers to assert complete meaning hypotheses, it is obscure exactly *which* complete meaning hypotheses they assert. Lexicographers do not formulate the complete meaning hypotheses explicitly. They have never given a survey of *which* ways of formulation are considered acceptable for a description of the claims of a dictionary.

5. ORIGIN AND CONTROL OF HYPOTHESES

The distinction between origin and control of hypotheses is well known from general methodology ²⁾. Let us try to make it clear with an example from English grammar, taken from O. Jespersen: *Essentials of English Grammar*, London 1933.

On p. 114, in the section on the direct and indirect object, Jespersen writes the following.

(1) As will appear from all the examples given hitherto, the rule, which is almost universal, is to place the indirect before the direct object.

This sentence (1), in the first place, does not express a definition, but a hypothesis. In order to *formulate* the hypothesis expressed by the sentence (2): "In English one generally places the indirect before

¹⁾ A short list of literature may be found in *Lingua*, 4, 423-4.

²⁾ See e.g. M. R. Cohen & E. Nagel: *An Introduction to Logic and Scientific Method*, London 1934, p. 273 ff., esp. p. 275.

the direct object" one, of course does not need to have performed research of any kind.

One might become acquainted with (2) by reading or hearing it, or it might be suggested to one after observing that in some English sentences the indirect object preceded the direct object.

Definition: With the *origin of a hypothesis* we refer to events and situations of any kind which cause a person to hit upon the hypothesis in question. Instead of the term "origin" we will also sometimes use the term "source", especially in plural.

Whatever may be the origin of a particular hypothesis, the fact that it has been formulated does not imply that it has been controlled. The way of testing a hypothesis is different from the way in which it comes into existence. The certainty of our knowledge of the truth of a particular hypothesis, depends amongst other conditions upon the amount in time and quality of testing-activities which have been performed in order to decide its truth.

A grammatical text-book, of course, does not give a satisfactory account of the total amount of research-activity which has been spent in order to *control* the hypotheses which are asserted within its covers. The examples in grammatical text-books are mainly of value for *pedagogical* purposes, they are quite unsatisfactory for the purpose of *controlling* the grammatical hypotheses which are asserted in the text-books. The function of examples is to *illustrate*, not to *control*.

The 20 examples which are offered on p. 114 in Jespersen's text-book of English grammar *illustrate* (in other words: *clarify the meaning of*) the hypothesis that in modern English the indirect object precedes the direct object, except in the case of weak pronouns. Whether the 20 examples also may be considered valuable for the purpose of *controlling* the same hypothesis, depends upon the question whether the examples are a representative sample of that kind of English which Jespersen attempted to describe.

6. ORIGIN OF MEANING HYPOTHESES

In this section we will survey the main ways in which meaning hypotheses come into existence. The attention will especially be focused on the definientia. A classification of the sources of definientia will be proposed.

Let us consider the noun "anlace". The following definiens is given in Wyld's English dictionary: "A short dagger with broad, tapering blade, formerly worn in the girdle". Let us suppose that the intended definiens has only been "a short dagger with broad, tapering blade", and that the phrase "formerly worn in the girdle" does not belong to the definiens, but has been included for the purpose of supplying additional information of interest for the identification of anlaces.

The preface of Wyld's dictionary justifies to enlarge the above rudimentary meaning hypothesis into the following complete meaning hypothesis: (3) In contemporary literary and colloquial English the noun "anlace" denotes the same as "short dagger with broad, tapering blade".

More exactly, the preface justifies the particular form indicating the field of validity (i.e. "in contemporary literary and colloquial English"), but the preface does not justify the inclusion of the particular meaning relational phrase which has been chosen, namely "denotes the same as".

The definiens of meaning hypothesis (3) may have been suggested to Wyld in several ways, which I propose to classify under the following 4 headings.

- a) The definiens may have been suggested to Wyld by an older dictionary.
- b) The definiens may have been suggested to Wyld through his intuitive command of the English language.
- c) The definiens may have been suggested by the excerpts.
- d) The definiens may have been suggested to Wyld by a collaborator or other person (e.g. an expert in different kinds of knives).

The above fourfold classification does not imply that a definiens which has come into existence has its origin in one, and only one, of the four sources. Several of the sources may contribute to the final phraseology of a definiens. A first suggestion from intuitive command of the language may result in a preliminary way of formulating the definiens. The preliminary way of formulating the definiens may be changed after consulting an earlier dictionary.

Whereas the classes a), c) and d) are fairly clearly circumscribed, the class b) comprises an unsurveyable number of situations. The intuitive feeling, as to the exact meaning of a form in a given context is the result of a number of experiences, most of which forgotten, and

the present context of the word. There is undoubtedly a need for splitting up class b), but how one could achieve such an aim, I do not know.

Even if the proposed classification of the possible origin of different *definientia* temporarily is accepted as satisfactory, it is to be noted that often it is not practically possible to know to *which* of the four sources a *definiens* is due or whether it is due to any of them. Even if the suggestion for a *definiens* comes during the study of the excerpts, this is no guarantee that the excerpts constitute the origin of the *definiens*. Perhaps it would have sprung up in the mind of the lexicographer anyway.

Sometimes the *excerpts* may include a sentence expressing a rudimentary or complete meaning hypothesis. In such cases the *wording* of the *definiens* is suggested by the excerpts. But other times the study of the excerpts may result in a *definiens* with a wording which is not identical with the forms occurring on the excerpt-slip.

One might perhaps believe that if a dead language were the object of study, source *b* (the intuitive command of the language) could not possibly be the source of a given *definiens*. But if a lexicographer has an active and idiomatic command of the language in question, one may expect that many a *definiens* may be due to the intuitive source.

One may note that the excerpts in lexicographical research are used both as a source for possible *definientia* and as material for controlling the meaning hypotheses. As a *source*, the excerpts constitute only *one* of 4 possible sources. *For controlling purposes*, however, *the excerpts provide the only available material*. If it is objected that sometimes lexicographers themselves produce sentences for control purposes, then it may be answered that these may be considered as being included among the excerpts.

Let us suppose that a given meaning hypothesis *M* is asserted to have its origin in class c). By our definition of "origin", this implies that the cause of *M* is in c). Accordingly, any obscurity attaching to the notion of cause is attached to our notion of origin.

In view of the elaborate philosophical discussions on the notion of cause and the uncertainty attached to the conditions of control of a causal statement, one might consider it preferable to avoid this notion. The main reasons for not doing so, can be formulated in the following way.

First, by using the notion of cause one does not commit oneself to

any particular philosophical theory. Any consistent theory can still be accepted. Secondly, there is a large number of different scientific theories for which the conditions of control are insufficiently known. But the theories are accepted nevertheless. That the conditions of control of a theory are sufficiently known, is the exception, not the rule. Thirdly, the definiens of "origin" strikes in my opinion an important direction of preciseness of the term, a direction preferable to other directions, and to which I will therefore stick here.

7. CONTROL OF MEANING HYPOTHESES

All hypotheses, and hence meaning hypotheses, have the property that they may be false or true. Falsehood and truth are called "truth-values".

In the following I survey a number of factors which are characterized by the property that changes in the factors always or sometimes are accompanied by a change in the truth-value of meaning hypotheses. Tentatively the factors are classified under 3 headings, A, B and C.

The first group of factors (A) is concerned with the nature of the evidence which is used to prove or disprove meaning hypotheses.

The second factor (B) is concerned with that particular form of the meaning hypothesis which indicates the field of validity.

Thirdly (C) we survey the influence of selecting a particular meaning relational phrase upon the truth-value of the meaning hypothesis.

A. On the Nature of the Evidence of Meaning Hypotheses

One of the characteristics of the material which is used as evidence in lexicographical research is that only if the excerpts are *interpreted* they may serve as evidence for or against a particular meaning hypothesis. What (if anything) occurs when a linguistic form is interpreted is a subject of controversy among semanticists.

Considering the controversial status of interpretation in semantics, one might propose not to postulate interpretation as a scientifically useful concept.

Although I do not deny (but neither assert) that this is feasible in some parts of semantical research, it is hardly avoidable to make use of a concept *interpretation* (or a closely similar concept with another

name) in a satisfactory description of the relation between the evidence of meaning hypotheses and the meaning hypotheses.

Let us suppose that the following rudimentary meaning hypothesis (4) is given:

(4) "Anlace" denotes the same as "short dagger with broad, tapering blade",

and the following excerpt:

(5) In the moat they found the body of a man with an anlace stuck up to the hilt in his breast.

(5) may be uttered or written. The mere hearing or seeing of (5) is not, however, sufficient for making (5) evidence for (4). A necessary condition for (5) being evidence for (4) is that both (5) and (4) are *interpreted*. Other expressions suggesting the same meaning to me as "interpret" are: understand, attach a meaning to.

Supposing that a lexicographer wants to describe the meanings of a word "a" in current usage, the following may happen. The sources of the meaning hypotheses supply the lexicographer with a number of candidates for *definienda*. Among the *definiens* candidates the lexicographer selects those which in his opinion come closest to the meanings of "a" in current usage.

But how does the lexicographer conclude that a list of *definienda* "b1", "b2" ... "bn" come closest to the meanings of "a"? There is no other way than through interpretation of both "a" and the *definiens* candidates.

Any meaning hypothesis may be proved by choosing an identical interpretation for definiendum and definiens. No matter what interpretation we choose for "anlace" in (5), if we choose the same interpretation for the *definiens* in (4), i.e. "short dagger with broad, tapering blade", then (5) provides strong evidence – but not necessarily conclusive evidence – for the truth of (4).

It is practically possible to interpret "anlace" in (5) so as to disprove (4), or more exactly, to disprove an interpretation of (4). Therefore, *the amount of evidence for a given meaning hypothesis depends upon the interpretations given to the excerpts and the sentence used to express the meaning hypothesis.* The truth-value of the meaning hypothesis is dependent upon the chosen interpretations.

If a person *A* states that a particular excerpt confirms a meaning hypothesis, whereas another person *B* states that it disconfirms the

same hypothesis, these statements may be due to their attaching different meanings to the definienda or the definientia or both. The statements of both *A* and *B* may be true, if their interpretations of the excerpts differ.

If an excerpt and a sentence expressing a meaning hypothesis are interpreted in exactly the same way by two persons testing the hypothesis, then the excerpt for these two persons presents exactly the same amount of evidence in relation to the meaning hypothesis. If their interpretations differ then the excerpt may or may not for these persons present exactly the same amount of evidence in relation to the meaning hypothesis.

Although the truth-value of a meaning hypothesis depends on the interpretations of the linguistic forms which are used for evidence and on the interpretation of the sentence which is used to express the meaning hypothesis, we do not know whether the interpretations of lexicographers who work on the same dictionary article *do* vary. Arne Næss ³⁾ has worked out scientific tools whereof some, perhaps, may be used to find out whether the interpretations of lexicographers differ.

B. The Field of Validity of Meaning Hypotheses

On p. 295 I stipulated that a complete meaning hypothesis comprises 4 forms, among these the form of the field of validity. In my terminology to any empirical hypothesis belongs a field of validity, that is a domain where information relevant to the truth-value of the hypothesis may be found.

The exact field of validity of a meaning hypothesis is determined by the wording of the hypothesis and how this wording is understood by interpreters. Hence for different interpreters the field of validity of a hypothesis may or may not be identical.

Let us consider the following two meaning hypotheses (6) and (7):

(6) "Anlace" as understood by Wyld when he was engaged in writing the article on "anlace" means the same for educated Englishmen as "short dagger with broad, tapering blade" as this phrase was understood by Wyld when he was engaged in writing the article on "anlace".

(7) "Anlace" as understood by Foster Dulles when asked the 5th

³⁾ See esp. A. Næss: *Interpretation and Preciseness*. Oslo, 1953.

of May 1955 means the same for educated Englishmen as "short dagger with broad, tapering blade" as understood by Dulles on the same occasion.

I assume that the readers of this article interpret (6) and (7) so as to make their fields of validity different in important respects.

The way in which Dulles interpreted "anlace" and "short dagger with broad, tapering blade" on the said occasion is relevant for the testing of (7), but not of (6). The way in which Wyld interprets "anlace" and "short dagger with broad, tapering blade" are, however, relevant both for (6) and (7), provided that we include Wyld among the educated Englishmen. But certain situations which are required by (6), namely interpretations made when writing the said article on "anlace", are not required for (7).

The preceding remarks show sufficiently that the truth-value of a meaning hypothesis depends upon what exactly is selected as its field of validity. This being so, an explicit formulation of a meaning hypothesis will make it easier to decide whether independent research workers are controlling the same hypothesis.

C. *Interchanging Meaning Relational Phrases*

In actual lexicographical research lexicographers take the utmost care in the selection of the lists of *definientia* in dictionary articles. In striking contrast to this care is the almost complete neglect in explicitly formulating the particular meaning relational phrases which have been used in the construction of a dictionary.

Let us consider the following list *L* of meaning relational phrases where the open spaces ... may be used to insert definienda and definientia.

1. "... means the same as ..."
2. "... denotes the same as ..."
3. "... designates the same as ..."
4. "... expresses the same as ..."
5. "... is used in the same way as ..."
6. "... is synonymous with ..."
7. "... has the same sense as ..."
8. "... has the same signification as ..."
9. "... is interpreted identically with ..."
10. "... may be defined as ..."

11. "... is understood in the same way as ..."
12. "... may also be formulated as ..."
13. "... is often abridged to ..."

It would be easy, for English only, to obtain a list of 50 meaning relational phrases. Many of these would, of course, differ only slightly in wording.

The neglect of lexicographers in specifying the particular meaning relational phrase (if any) which has been used is justifiable, if at least one of the following two sufficient conditions C1 and C2 hold.

- C1. The selection of a particular meaning relational phrase A from the above list L rather than another B, does not make any lexicographer adopt other definientia than those which had been selected.
- C2. The interchange of meaning relational phrases of L does not in the opinion of lexicographers lead to changes in the truth-values of the complete meaning hypotheses in which the meaning relational phrases occur.

Even if both C1 and C2 hold, it is not unreasonable to call the lack of an explicitly formulated *list* L a "methodological neglect". It is an important task for the methodology of lexicography to construct such lists L for dictionaries of different languages, and to attempt to find out whether interchangings of the members of these lists have effects upon the selection of the definientia or upon the truth-values of meaning hypotheses.

The above list L may at best only be used for English. But it is practically possible to construct for each spoken and many dead languages a list of forms which may serve as candidates for meaning relational phrases. And interchangings of the members of these lists may or may not satisfy C1 and C2.

It is tempting to make a guess as to whether conditions C1 and C2 would be satisfied if L was used in relation to research workers engaged in constructing English monolingual dictionaries. I am inclined to believe that only rarely would the interchangings result in interchangings of definientia or in changes in the truth-values of complete meaning hypotheses.

In section 7C it has been my aim to expose the results which interchangings *may* have. Whether interchangings *have* such results, cannot be decided by pure reasoning.

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ANNOTATIONS TO MR. TURNER'S "AFRICANISMS IN THE GULLAH DIALECT" (1949)

"Certainly no one can now safely say that Gullah is merely a combination of 17th century British dialects and baby talk (cf. p. 5), somehow mangled by the inept articulation of ignorant and lazy savages. Once more anthropological training and linguistic method have dissipated a myth". This striking sentence forms the conclusion of an earlier review ¹⁾ of the important publication to be discussed in the following pages. Already a year earlier the author had published a preliminary study on Gullah ²⁾, perhaps the only example of creolisation in the United States. Gullah is spoken by the Negro population of the coastal belt of South Carolina and Georgia (p. 3), who after 1865 and the final abolition of servitude in 1907 have been able to maintain in their comparative isolation ³⁾, both culturally and geographically, an excellent example of linguistic creolisation in its pure form against attempts at unification undertaken by the "official" standard language. The name *Gullah* and its equivalent *Gitsi* go back to names of tribes in present-day Liberia (pp. 90 seq., 194).

Due to many earlier publications we have already obtained some insight into Negro-Dutch, Negro-English etc., and in the technical process of linguistic creolisation in general ⁴⁾. How many component

¹⁾ Raven I. Mac David in *Language* XXVI² (1950), pp. 323 seq. For other reviews of Mr. Turner's book see *Linguistic Bibliography for the Year 1950* (Antwerpen, 1952), p. 153; *ibid.* for the Year 1951, p. 160 and *ibid.* for the Year 1952, p. 274; Rob. A. Hall in *Amer. Speech Quarterly* (New York, XXV, 1950), pp. 51 seq.

²⁾ L. D. Turner, *Problems confronting the Investigator of Gullah* (Publ. Amer. Dialect Soc., Greensboro, N.C. IX, 1948), pp. 74 seq. and *id.*, *Notes on the Sounds and the Vocabulary of Gullah* (*ibid.*, III 1945), pp. 13 seq.

³⁾ This is shown i.a. by the fact that "many English relic-forms seldom heard elsewhere in North America should be found in Gullah" (Mac David, l.c., p. 325).

⁴⁾ Turner's index contains an extensive survey of the literature concerning creolization. For the neighbouring linguistic areas see i.a.: R. A. Hall, *Haitian Creole* (Mem. Amer. Anthropol. Association LXXIV, 1953); C. H. de Goeje, *Etudes linguistiques Caraïbes* (Verh. Kon. Akad., A'dam 1937 and 1946); D. C. Hesseling, *Het Negerhollands der Deense Antillen* (Leiden, 1905); Lichtveld, *West-Indische Gids* X (1928/29) and XI (1929/30): Een onderzoek naar

linguistic parts may finally lead to a creolised synthesis, producing a literature of its own and consequently a new and productive linguistic unity, is proved by the Papiamentu of the Netherlands West-Indian territories, in the first place by Curaçao and Surinam. Although shortly before the beginning of the nineteenth century the Dutch element comes to the fore, and Spanish soon afterwards, the basis for this creole language had been laid down by the Afro-Portuguese, the Coastal Portuguese of the African littoral being still strongly represented in the syntax and especially in the vocabulary ^{4a}). Although in view of this single example, a strong Portuguese basis, e.g. from Angola, might likewise be expected for Gullah, the extensive list of names and the vocabulary collected by Turner only contain thirteen etyma which may be traced to this source, and even in this case their Afro-Portuguese origin is still quite problematical. The nearly complete absence of this lingua franca of the slaves in the Gullah region therefore requires further historical research.

Turner's description of Gullah is a shining example of the systematic description of a language which is far from being "pidgin", but which

Afrikaansche „resten" in de Creolentaal van Suriname; J. Voorhoeve, Voorstudies tot een beschrijving van het Sranen-Tongo, het Negerengels van Suriname (A'dam, 1953); J. P. B. de Josselin de Jong, Het huidige Negerhollandsch (Danish-Dutch Archeol. Exped. to the Antilles, 1922-1923, Verh. Kon. Akad., A'dam XXVI, 1926). For a summary of older studies see H. Schuchardt in J. Vendryes, *Le langage* (Paris, 1921), pp. 330 seq.; O. Jespersen, *Language* (London, 1923), Chapt. XXIII; John B. Carroll, *The Study of Language* (Harvard Univ. Press, 1953), p. 58; L. Bloomfield, *Language* (New York, 1948), p. 474; U. Weinreich, *Languages in Contact* (New York, 1953); H. Schuchardt-Brevier (ed. L. Spitzer, Halle, 1928), pp. 150 seq., etc. Cf. finally *Language* XIII (1937), pp. 148 seq., and for the problem of related creolization G. S. Lane, *Notes on Louisiana French* (*Language* IX, 1935, pp. 5 seq.); E. Jourdain, *Du Français aux parlers Créoles, suivi d'un Vocabulaire du parler créole de Martinique* (1956); *id.*, *Le verbe en créole martiniquais* (*West-Indische Gids* XXXV, 1954, Den Haag, pp. 39 seq.); H. E. Funk, *The French Créole of Martinique* (Univ. Virginia Diss., 1953) and F. Ortiz, *Glosario de Afronegrismos* (Havana Imprenta El Sigla, XX, 1924).

^{4a}) See the important public lecture by H. L. A. van Wijk (8 February 1937). For Papiamentu cf. i.a. R. Lenz, *El Papiamento* (Santiago, 1928 with bibliography); G. P. Jansen, *Diccionario Papiamentu-Hollandés* (Curaçao, 1948); E. Goglio, *Gramatica Papiamentu* (Curaçao, 1953); most of these authors, however, stress the Spanish rather than the Lusitanian share in the bases of Papiamentu.

has to be considered as a phonological and structural unity with its own syntax and its own vocabulary, to be traced to the unification of "base languages" with the "native language" based on a system of structural unity⁵). Here the difference between "pidgin" and creolisation is clearly apparent, distinguishing it fundamentally from the large and more or less indefinite group of *linguae francae*, etc.

Turner's work is especially concerned with the linguistic material coming from West Africa (see the map on p. 7), which the negroes taken as slaves to America carried with them as an unalienable cultural possession and which they have preserved so well ever since that it has proved possible to determine the origin of the linguistic data with a certain degree of probability even to-day.

The work is divided into nine chapters. Here the author describes 1°. the historical background of the "Gullah"⁶) population, and the difficulties for scholarly research (pp. 1—14); 2°. the "phonetic

⁵) Rob. A. Hall. *Pidgin English and linguistic Change* (*Lingua* III, 2, 1952), pp. 138 seq; in general Einar Haugen. *The Analysis of linguistic Borrowing* (*Language* XXVI, 1950), pp. 120 seq.

⁶) See note 3 above. Already the name itself points geographically in the direction of the Western Sudan (Wolof, Malinke, Mandinga, Bambara, Fula, Mende, Vai, Twi, Fante, Gã, Ewè, Yoruba, Efik, Hausa, Ibo, etc.), i.e. to the region which extends from Senegal to Kamerûn, as being the most important centre of the export of slaves. See the most recent work by Dr J. W. Schulte Nordholt, *Het volk dat in duisternis wandelt* (Arnhem, 1956), with an extensive and thoroughly documented survey of the slave trade. Mr. Turner gives some native Gullah texts on this subject (pp. 281 seq.). John Gunther, *Inside Africa* (1956), in his ch. 29 and 36—45 deals very superficially with the earlier history of the African tribes he discusses. For the slave trade inside Africa see i.a. H. Baumann, R. Thurnwald & D. Westermann, *Völkerkunde Afrikas* (Essen, 1940), pp. 136 seq., 243 seq., 334 seq. In this connection, H. M. Stanley, *In darkest Africa* (1897). p. 31, and D. Westermann, *Geschichte Afrikas* (Köln, 1952) stress the importance of Tippu-Tib (as does H. Brode, *Tippo-Tib, The Story of his Career in Central Africa* (London, 1907); *id.*, in *MSOS* 1902, pp. 175 seq. and 1903, pp. 1 seq; H. P. Blok, *A Swahili Anthology* (Leiden, 1948, pp. 9 seq.). Among the first explorers who in about 1870 pointed to the existence of the problem of slavery inside Africa, mention must be made of G. Schweinfurth, who in his *Im Herzen Afrikas* (Jubil. ed. Leipzig, 1918, pp. 449 seq.) devoted a long chapter to this problem. According to Dr. A. Prins, *Tanganyika* even owes its name to the fact that it was here that the collecting-point for the eastern slave caravans was to be found (*Meded. Afrika Inst.*, Rotterdam, X, 1956, p. 86).

alphabet" and the diacritical marks used to indicate accent, tonology, glottalisation, nasalisation, length, etc. (pp. 15—30); 3°. West-African words and expressions preserved in Gullah (pp. 31—209); 4°. syntactical peculiarities of Gullah (pp. 209—223); 5°. morphological characteristics (pp. 223—231); 6°. word formation (pp. 232—239); 7°. "sounds", i.e. the system of consonants and vowels (pp. 240—248); 8°. intonation (pp. 249—253), and 9°. Gullah texts in phonetic transcription, with translation (pp. 254—289). A very extensive bibliography (pp. 293—309) with additional notes concerning further details concerning both linguistics and social anthropology conclude the work.

I may be allowed to make a few remarks concerning both the division and the contents of the book, but these in no way detract from the high regard I have for the method and the material, which ensure Turner's work a prominent place in view of the treatment of a subject so difficult to handle and so little described.

In my opinion there is a serious objection against the division of the book as outlined above. The chapters which describe the "phonetic alphabet", i.e. the phonemes, the tonemes and the "sounds" (again phonemes) are scattered at present and so they do not provide one comprehensive survey of the phonological system. The structural factors, discussed here separately in the unsystematic order of syntax — morphology — word formation, have been taken up in an arbitrary fashion. They are not immediately connected with a uniform description of the phonemic inventory valid for Gullah. The connection between these two fundamental bases of language is suddenly interrupted by the lexical material mentioned sub 3., and this latter is therefore not linked up immediately with the collection of texts either. In my opinion the two chapters 3 and 9 ought to follow the theoretical part by way of illustration, after the systematically arranged material which Gullah provides both phonologically (nos. 2, 7 and 8), and structurally (nos. 4—6). For that matter, the place of the *stylistic* paragraph: "Frequent repetition of words and phrases throughout the sentence" (pp. 220 sq.), as a transition from the survey of the syntax to that of the morphology is likewise surprising.

I would like to start with that part which Mr. Turner has called *the sound system*, i.e. the inventory of the phonemes he found. It is in creolisation that we observe the "economy of phonetic change"

stressed by A. Martinet ⁷), an economy which becomes apparent most clearly in case of creolisation because there the diachronic conservatism proves "how stubbornly a language resists radical tampering with its phonetic pattern" ⁸). In this connection it deserves notice that several phonological phenomena in Gullah can only be explained against the background of the African mother-tongues, as e.g.:

the nasalised initial cluster with a homorganic stop, the "rolled lingual *r*" peculiar to the phonetic system of the languages of the Western Sudân (p. 28), the retroflex flap /ɾ/, which also indicates an African origin (p. 244) ⁹), and the implosive consonantal phonemes which are above all peculiar to the African languages (pp. 22 sq.). In Bantu ¹⁰) these are restricted to the eastern and southern areas (In-

⁷) A. Martinet, *Economie des changements phonétiques. Traité de phonologie diachronique* (Berne, 1955).

⁸) E. Sapir, *Language* (New York, 1921), p. 211; *id.*, *Sound Patterns in Language* (Language I, 1925, pp. 37 seq.). Although it is concerned with completely different groups of languages, I would like to refer to a book which is also of importance for general linguistics, viz. E. Petrovici, *Kann das Phonemsystem einer Sprache durch fremden Einfluss umgestaltet werden? Zum Slavischen Einfluss auf das rumänische Lautsystem* (Den Haag, 1946).

⁹) Cf. C. T. Hodge, *An Outline of Hausa Grammar* (Lang. Diss. XLI, 1947), p. 11, already in F. W. Taylor, *A practical Hausa Grammar* (Oxford, 1923), § 8; see also *Language* XXVI² (1950), p. 332 note 40; D. Westermann and I. Ward, l.c., §§ 207 seq.; H. Zwaardemaker and L. P. Eykman, *Leerboek der Phonetiek* (Haarlem, 1928), pp. 202 seq.; for the Bantu area: A. N. Tucker, *The comparative Phonetics of the Suto-Chuana Group of the Bantu Languages* (London, 1929), pp. 47 seq.; A. Burssens, *Inleiding tot de Studie van de Kongolese Bantoetalen* (Kongo-Overzee Bibl. VIII, Antwerpen, 1954), § 62; etc. It should be stressed that "the various intermittent *r*-sounds have the vocalic as well as the consonantal feature", see R. Jakobson, C. G. Fant and Morris Halle, *Preliminaries to Speech Analysis* (Mass. Inst. of Technol., Report XIII, 1952), p. 19.

¹⁰) Cf. D. Westermann & I. Ward, *Practical Phonetics for Students of African Languages* (Oxford Univ. Press, 1949), pp. 92 seq.; Cl. M. Doke, *Bantu Linguistic Terminology* (London, 1935), p. 122; L. Bloomfield, l.c., pp. 97, 119; E. Fischer-Jørgensen in *Acta Linguistica*, VII (1952), p. 28; in general i.a. F. de Saussure, *Cours de Linguistique générale* (Paris, 1949), pp. 79 seq., 90; N. S. Trubetzkoy, *Principes de Phonologie* (ed. J. Cantineau, Paris, 1949), pp. 135 seq.; R. Jakobson and Morris Halle, *Fundamentals of Language* (Den Haag, 1956), pp. 34 seq. For the problem of bilabial and alveolar implosives in Bantu-languages see C. Meinhof, *Bantu Phonology* (ed. N. J. van Warmelo, Berlin, 1932), pp. 28 seq.; Cl. M. Doke, *Textbook of Zulu Grammar* (London), 1945,

hambane, Shona, Nguni), but in the Sudân languages they are to be found in the labio-velars *k'p* and *g'b*, as well as in *b*, *d* (e.g. in Hausa) and to a lesser degree in the semi-consonant *'y*¹¹). There proves to exist a close relation between the vowel system of Gullah and of the West African idioms, Ewè and Yoruba in the first place (pp. 16 sq. and 246 sq.). Moreover, Gullah possesses a comparatively small number of vowel phonemes, a distinguishing trait which it has in common with West African languages.

The highly important problem of the intonation the author treats very briefly (pp. 249 sq.). In his short survey semantical and syntactical factors are confused, at the expense of the latter. There is no mention of pitch-phonemes occurring in morphemes on the one hand, and stress on the other, which may or may not be phonematic, as studied by Olmsted for Yoruba and Lucumí¹²), a phenomenon of vital importance for the "technics" of creolisation. We express the hope that we may be allowed to see a further study on this point by this expert author supported by many informants.

As mentioned above, the description of the phonological material (chapters 2, 7 and 8) is interrupted by a number of word-lists (see

pp. 8 and 11; *id.* The Southern Bantu Languages (ed. IAI, Oxford Univ. Press, 1954), pp. 31 seq.; *id.*, Outline Grammar of Bantu (Johannesburg, 1943), pp. 13 seq.

¹¹) See D. Westermann and M. A. Bryan, Languages of West-Africa (ed. IAI, Oxford Univ. Press, 1952), pp. 9, 170. Implosive *ɓ*, *ɗ* and *ɟ* also occur in the north-eastern non-Bantu languages, see A. N. Tucker and M. A. Bryan, The non-Bantu Languages of north-eastern Africa (ed. IAI, Oxford Univ. Press, 1956), pp. XIII et passim. Implosive *ɣ* in Hausa is not mentioned by D. Westermann and I. Ward, Practical Phonetics etc. (see note 10), § 276.

¹²) C. David L. Olmsted, Comparative Notes on Yoruba and Lucumí' (Cuba), in Language XXIX² (1953), pp. 159 seq. (with literature). See also Raven I. Mac David, l.c. p. 331 and in general R. Jakobson and Morris Halle, Fundamentals of Language (Ianua Linguarum, Den Haag, 1956), pp. 22 seq. As to intonation tonality does play an important role in phonematics and prosodemics (see E. Haugen in Language XXV, 1949, pp. 278 seq.; D. Jones in Acta Linguistica IV, 1944, pp. 40 seq.; J. R. Firth in Transact. Philol. Soc. London, 1949, pp. 107 seq.; cf. D. Jones, The Phoneme, Cambridge, 1950, pp. 114 and N. S. Trubetzkoy, Principes de Phonologie, Paris 1949, pp. 212 seq.); description of Gullah: "whereas the Gullah dialect does not have significant tone in the sense in which the West-African tone languages do, it does have a characteristic intonation and rhythm". (Turner pp. 30, 249 seq.).

below, sub II) and remarks concerning syntax (pp. 209 sq.), morphology (pp. 223 sq.) and word formation (pp. 232 sq.). Here we observe many cases of a use of language which is related to West African idioms, but which differs from American English: the lack of a verbal passive; the preference for parataxis over against hypotaxis necessitating the use of auxiliary verbs for rendering "our" participles, conjunctions, etc.; the use of *dă* ("to be") in all tenses for incomplete predication; circumlocation of the superlative by means of a verb (*pas* = "to surpass"); the type of predicate qualified by Turner as "verbal adjective", a moot point where the choice between the verbal and the nominal function could only be decided via a structural analysis of the language under discussion (pp. 215 sq.). This holds good all the more because in Gullah the adjective may immediately follow the noun in the attributive function and not in that of the "verbal adjective", e.g. in *rol răuñ* (lit. "roll round", i.e. "a round roll, a bisquit" ¹³)).

Among the morphological characteristics which approach Gullah to its African precursors, there is to be noted the lack of nominal formatives of gender and number. In the latter case the addition of a numeral or of a pronoun in the plural (unless it be in the possessive form, p. 229) is necessary; in the first case the noun is preceded, in order to distinguish the gender, by *ūmă*, "woman", or *man*, sufficiently known from the groups of African languages. The fact that the verbs do not know tenses in our sense of the word, but are exclusively restricted to the expression of aspects *qua tales* without any difference in "time" (pp. 225 sq.), is in complete agreement with the African origin of Gullah. Here the absence of a differentiation in "case" is normal, viz. the absence of a morphological difference between the subjective, the objective and the possessive function in the pronominal system, which, however, does possess both a singular and a plural; it is only the 3rd person singular which knows a difference in gender in the objective and in the possessive function (*him*, *ēm* and *ši*). In the same way, *mutatis mutandis*, e.g. also in Ewè and Gã, in contrast with the above mentioned lack of plural morphemes, a difference in number is only made in case of a possessive connection between two nouns, but

¹³) In view of the available typographic facilities the transcription of Gullah has been slightly simplified in this review.

"Gullah nouns have the same form in all cases" ¹⁴). Also the absolute and the connective forms of the personal pronoun, found e.g. in Ewè, are lacking in Yoruba and Ibo, as well as in Gullah.

The next chapter (6, pp. 232 sq.) discusses several ways of word-formation, parallels of which may be found in the West African languages. Apart from verbal forms like *pukpula* (Mende: *kpukpula*, "to cause diarrhoea", p. 150) etc., either partial or complete reduplication of the root of the word constitutes an integral factor in intensification and in the formation of adverbs, etc.

Turner ends his chapter on "Word-formation" with a list of onomatopoea where quite naturally reduplication plays an important part. However, under this heading he combines both the "sound onomatopoea" (i.e. especially in the names of animals), and what Cl. M. Doke has quite rightly called "ideophones" ¹⁵). These are not direct imitations of animal and other sounds, but "the vivid representation of an idea in sound. A word, often onomatopoeic, which describes a predicative, qualitative or adverb in respect to manner, colour, sound, smell, motion, state or intensity", with an accent and an intonation of its own (cf. Turner, p. 149, *pitipa*, Mende; *tititi* intensive, p. 205; *pinëpinë*, "quickly", p. 207, etc.). Hence I do not think it correct that Turner treats the ideophones on the same level as the more general and concrete onomatopoea. Still, in Gullah and the languages of the Western Sudân mostly reduplicative formations are concerned, whereas in Zulu, for instance, these only form part of the ideophonic system (c.f. Cl. M. Doke, Text-Book, § 644). In any case, ideophony is much more richly represented in the area of the Bantu languages than in the group of the languages of the Western Sudân. Reduplication as a means to intensify the meaning occurs especially in Gullah personal names (pp. 43 sq. and 235), even in cases where there is hardly any question of ideophony.

¹⁴) Turner, l.c., pp. 223, 229.

¹⁵) Cl. M. Doke, Bantu Linguistic Terminology (London, 1935), p. 118; *id.*, Text-book of Zulu Grammar (London, 1945), § 625, mostly with initial stress; *id.*, Outline Grammar of Bantu (Johannesburg, 1943), pp. 56 seq.; Cf. J. Marouzeau, Lexique de la terminologie linguistique (Paris, 1951), p. 114.

II

The two chapters III and IX — whose separation I believe to be unjustified — provide very important material; on the one hand consisting of personal names, words and expressions obtained from conversation and oral tradition, on the other of a series of complete Gullah texts with widely varying contents. The main part of Turner's work is devoted to an enumeration of personal names (pp. 30–190), preceded by a short survey of the formation of names among the tribes of the Western Sudân ¹⁶⁾ and a few words concerning this process in the Bantu area of Kimbundu (Angola). This is somewhat surprising, because the Western Bantu languages (like Bobangi, Kikongo and Kimbundu in Angola, and TshiLuba, etc.) are amply represented among the etyma which Turner adduces in explanation of the Gullah personal names. Now the interpretation of personal names ¹⁷⁾ which rather belong to the exclamatives and metaphorical descriptives than to a class of grammatical-communicative words, constitutes a difficult etymological problem ¹⁸⁾. This is already to be observed from the way in which the author tries to reduce the personal names semantically to widely different idioms (Sudân and Bantu), often without being able to make a choice. The possibility of phonetic comparison (not identity) is often decisive. Here social anthropology is able to provide a solution in many cases naming the child after the day and the time of birth, naming it after the clan or the secret society to which one of the parents belongs ¹⁹⁾, or after social castes like court

¹⁶⁾ Especially derived from Efik, Wolof, Twi and Yoruba (see note 12 above); as regards the formation of word-names see in particular D. Westermann, *Die Sprache der Guang in Togo und auf der Goldküste* (Berlin, 1922), and the work by D. Westermann and M. A. Bryan, mentioned in note 11. Cf. also F. W. H. Migeod, *The Languages of West Africa* (London, 1913), II, pp. 339 seq.

¹⁷⁾ Cf. O. Jespersen, *Language* (London, 1923), p. 436; A. Gardiner, *The Theory of Speech and Language* (Oxford, 1932), pp. 41 seq.; A. Dauzat, *La Philosophie du Langage* (Paris, 1920), p. 309.

¹⁸⁾ J. Holloway, *Language and Intelligence* (London, 1951), pp. 28 seq.; C. K. Ogden and I. A. Richards, *The Meaning of Meaning* (London, 1953), pp. 213, 273 seq.

¹⁹⁾ Cf. e.g. E. J. Krige and J. D. Krige, *The Realm of the Rain Queen* (Oxford Univ. Press, 1947), pp. 87 seq.; H. Kuper, *An African Aristocracy* (ibid., 1947), pp. 110 seq. for the southern Bantu area.

dignitaries or even legendary rulers and ancestors, etc., well known from comparative anthroponomy. In this connection a few examples may be mentioned. Gullah, going back to Yoruba, has names like *yètũnde* (p. 185) and *babatũnde* (p. 57), given to a daughter resp. a son, after the death of the grandmother, or resp. the grandfather, as whose reincarnation the child is considered ²⁰). Exclamations of the mother on the birth of the first child (*kemu* Mende: "it is a boy!") and *kǎi* (Yoruba, admiring exclamation; pp. 108, 112) bring the giving of the name within the directly human sphere. In contrast to this we find pejorative names like *mbekele* ("dwarf"), *olugbodi* (Yoruba: "child born with supernumerary digits"), *diabu* ("devil", derived from Portuguese via Angola), *eleghara* (a name for the devil in Yoruba), *fwa* ("corpse"), *fyene* (Bambara: "blind"), *šaitan* ("devil", from Arabic via Hausa), *ohali* (Angola: "misfortune"), etc. We also find personal names referring to death, like *kufiya* (Ewè: "capital sentence", likewise an additional name for a goddess of death), *kurumi* (Yoruba: "death is destroying me"), *kusoluwa* (Yoruba: "to die in God's hand"), *egugu* (Yoruba: "mortuary cult"), *ebunze* (name of an evil spell), etc.

Theophorous personal names are very numerous: *nzambi* ²¹) and *zambi*, the widely known name of the supreme deity in the western Bantu region, occurs twice, but the great majority of these theophorous names points to deities of the western Sudân and of the Yoruba in particular ²²). Examples are *Ifa* ²³) and his caste of priests, *babalawo*

²⁰) R. S. Rattray, *Religion and Art in Ashanti* (Oxford, 1927), p. 318; R. E. Dennett, *At the Back of Black Man's Mind* (London, 1906), p. 268; C. K. Meek, *The Tribes of Northern Nigeria* (Oxford Univ. Press, 1925), II, pp. 32 seq., 80, 112.

²¹) Cf. R. E. Dennett, l.c., chapt. XVI and pp. 105, 116; L. Bittremieux, *Mayombsch Idioticon* (Kon. VI. Akad. v. Taal- en Letterk., Gent 1923) II, p. 545 with critical remarks on Dennett; K. Laman, *Dictionnaire Kikongo-Français* (Mém. Institut. Royal Colon. Belge II, Brussel, 1936), p. 821 etc.

²²) Cf. in general J. Parkinson, *Yoruba Folklore* (Journ. Royal Afr. Soc., VIII, 1906), pp. 165 seq.

²³) S. Johnson, *The History of the Yorubas* (London, 1921), pp. 32, 47; C. K. Meek, l.c., II p. 69; R. E. Dennett, l.c., pp. 232, 244 seq.; "Now *Ifa* means that which is stripped off, and the verb "to strip off" means also 'to create'. However, in the first place, *Ifa* is a divinity of divination, more magical than cosmic in character, in contrast to *Olorūn* (lit. "Lord of Heaven", see Turner, p. 141). It remains an open question in how far these Yoruba divinities were

and *iyalemöle*, *fatosiñ* (lit. "Ifa is worthy to be worshipped"), *fayemö* (lit. "Ifa honours the child"), etc. *Ifa* (Ewè: *afa*) is "the great consulting oracle in the Yoruba country and was introduced at a late period by king Onigbogi, who was said to have been killed for having done so"; his theophany takes place once every three years. The chief deities of the Yoruba, *Olorūn*, and *Legba*, however, still play an important role in the voodoo-cult of Central America, or rather in this secret society of the Negro population, both in the ritual and in the anthroponymy ²⁴).

Another deity to be found among the theophorous names is *Šango*, the god of thunder and lightning ²⁵), as his emblem already betrays, "smooth stones shaped like an axe-head and commonly taken for thunderbolts". Hence names like *Oše* ("cudgel of the god of thunder"), or, like this is done in the case of *Ifa*, names connected with the magical ritual concerned: *ewekoko*, *adošu*, etc. Related names are *Soja*, actually the name of the wife of the god *So* (also Gullah, Turner, pp. 161 sq.), which agrees again with the Yoruba personal name *Ōja* ²⁶).

In this connection two other figures, belonging originally to the mythology of Western Africa, may be mentioned, although they belong rather to the domain of folklore than to that of religion. The first is the fairy-like figure of "the Lady of the Brooks", *Yemaja* and her mother *Iyayemaja* (pp. 97, 144, 185) ²⁷). The female court-dignitary *emaja* (likewise a personal name, p. 80) occupies an elevated position at the court of the Yoruba ruler. It need not astonish us that in Africa the watergods occupy a special place. An example is furnished

taken over at some time or other from neighbouring Nigerian tribes (Popo, Akoko, etc.). A survey of the anthroponymy typical of the Yoruba is to be found in S. Johnson, l.c., ch. V.

²⁴) Turner, pp. 182, 204; Cf. i.a. J. J. Williams, Voodooos and Obeahs, Phases of West-India Witchcraft (London, 1933); Z. Hurston, Voodoo Gods. An Inquiry into native myths and magic in Jamaica and Haiti (London, 1939); H. Deschamps, Les religions de l'Afrique Noire (PUF, 1954), pp. 31, 41 and W. B. Seabrook, The Magic Island (London, 1931), pp. 276 seq., 282 seq., 303.

²⁵) Cf. C. K. Meek, l.c., II p. 41; S. Johnson, l.c., pp. 34, 149 seq.; R. E. Dennett, l.c., p. 245.

²⁶) For correspondences with Ewè cf. the terms *So-kpé* ("So's stone") and *So-fla* ("So's axe"), both parallels to Thor's "thunderbolt" (D. Westermann, Ewè-Deutsches Wörterbuch, Berlin, 1905, p. 440 and J. Spieth, Die Religionen der Eweer in Süd-Togo, Quellen der Religionsgeschichte, X, Göttingen, 1911, pp. 47, 177).

²⁷) See R. F. Dennett, l.c., pp. 245, 258, 270.

by the "primeval god" of the Bambara whose victory over the earthgod enabled him to create the cosmos from the chaos; he is "un être aquatique, mi-albinos mi-cuivre, androgyne; on se le représente volontiers comme une sorte de sirène avec une tête de femme blanche" ²⁸).

In the second place the reader's attention is asked for two very well-known folkloristic motifs from which Gullah personal names have been derived, viz. lycanthropy and the motif of the heavenly ladder, already known from the Old Testament. From Kimbundu Gullah derives the name *hitu* ("lion-man", cf. the Mandinga name *djato*). In the Gullah texts provided by Turner (p. 257) we find the motif of the "leopard-man", a motif widely distributed in the Western Sudân, which has its place in the ritual of many secret societies ²⁹).

The other motif to which I referred is that of the heavenly ladder, the connection of some kind between Heaven and Earth. It is to be found in antiquity, both in the Old Testament and in ancient Egypt ³⁰), and it may be recognised likewise in the mythology of all peoples. Everywhere the reunion of Heaven and Earth is symbolised, either by mechanical means (the ladder) or by a mythological interpretation of natural phenomena (the rainbow, etc.). It often possesses social or cultural traits, e.g. by being connected with the Prometheus-motif, where the fire from heaven is taken to the earth along the "heavenly ladder"; in the Western Sudân it is the Spider which plays the part of Prometheus (Dennet, l.c., p. 131). Parallel with these traits we find

²⁸) H. Deschamps, l.c., p. 31. Cf. the personal name *omiyale* (lit. "The god of streams visits the house", Turner p. 144).

²⁹) The literature on the "leopard-man" is very extensive; see i.a. Mary H. Kingsley, *West African Studies* (London, 1899), pp. 140, 165; H. Baumann, R. Thurnwald und D. Westermann, l.c., pp. 179, 328; B. Lindskog, *African Leopard Men* (Ethnogr. Stud. Upsal. VII; Uppsala, 1954); K. J. Beatty, *Human Leopards* (London, 1915); R. C. Dennett, l.c., pp. 144 seq.; J. Maes, *Aniota-Kifwebe* (Anvers, 1924), pp. 56 seq., fig. 56, 57; A. Le Roy, *La Religion des Primitifs* (Paris, 1911), p. 343.

³⁰) Genesis XXVIII, 12; see with comparative literature H. P. Blok, *Zur altägyptischen Vorstellung von der Himmelsleiter* (Acta Orientalia VI, 1928), pp. 257 seq. Cf. also R. C. Dennett, l.c., pp. 131, 193; L. Frobenius, *Die Masken und Geheimbünde Afrikas* (Nova Acta, Abhandl. d. Kaiserl. Leop.-Carol. Deutschen Akad. d. Naturforscher LXXVI, no. 1, Halle 1898), pp. 64 seq., 198, 263; J. Flatz, *Die Kulturen Ost-Afrikas* (Linz, 1936), p. 50. The oldest representation of the "Ladder of Salvation and the Heavenward Way" is to be found on the western wall in Chaldon Abbey (England, XIIth century).

the view that the deity lowered the first human couple he had created down to the earth by means of a bridge of spider-thread between heaven and earth ³¹). In Western Africa it is especially the spider which by means of its web establishes the contact between heaven and earth ³²) and which is therefore eminently qualified for the role of oracle ³³). As often happens, however, also in this case the myth gradually became legend and the legend grew into the popular tale. "In Obeah the spider (*anansi*) has a position given it equal to that of the snake in Voudou. Now the spider is all very well in Africa; round him there has grown a series of most amusing stories; but to put him on a place with the snake in Dahomey is absurd. His equivalent there is the turtle, also a focus for many tales, only more improper tales and not half so amusing" ³⁴). It is, however, in its very function as the weaver of the "heavenly ladder" made of thread, that the spider, beside being the main figure of the tales which resemble those of Reynard the Fox ³⁵), has been able to preserve the more ancient, legendary tradition and to find its way into the popular tales with a mythical foundation found in the folklore of far distant culture areas ³⁶). In view of these and similar considerations it is justifiable to include personal names like *anânse* (also in Surinam) and *nanse* (Curaçao and Gullah, see Turner, pp. 51, 198) likewise among the group of mythological, c.q. theophorous anthroponyms.

³¹) H. Deschamps, l.c., p. 32; A. F. MocKler-Ferryman, British Nigeria (London, 1902), p. 282; P. Güssfeldt, J. Falkenstein und E. Pechuël-Loeshe, Die Loango-Expedition (Stuttgart, 1907) IV, p. 267.

³²) P. Güssfeldt, a.o., l.c., IV p. 135; Baumann-Thurnwald-Westermann, l.c., p. 251; C. Meinhof, Afrikanische Märchen (Jena, 1921), pp. 155, 328 (Angola).

³³) E.g. M. Heepe, Yaunde Texten (Hamburg, 1919), p. 296.

³⁴) Miss Mary H. Kingsley, l.c., p. 140; cf. the Mantis-cyclis in South Africa.

³⁵) See i.a. C. Meinhof, l.c., pp. 207, 210 and several examples of this motif in M. van Loggem and O. Sterman, Meesters der Negervertelkunst (Amsterdam, 1956); cf. B. Cendrars, Anthologie Nègre (Paris, 1927), pp. 27, 217 seq.; M. Herskovits, Trinidad Village (New York, 1947), p. 127; N. van Meeteren, Volkskunde van Curaçao (Willemstad, 1947), p. 97; H. Hoetink, Het Patroon van de oud-Curaçaose samenleving (Leiden, 1958), pp. 97, 145; H. Deschamps, Les Religions de l'Afrique Noire (ed. PUF, 1945), pp. 24, 64; etc.

³⁶) See e.g. for Germanic folklore W. Mannhardt, Wald- und Feldkulte (Berlin, 1904) I, pp. 76, 120, and the related "Rapunsel"-motif in J. Bolte and G. Polívka, Anmerkungen zu den Kinder- und Hausmärchen der Brüder Grimm (Leipzig, 1913) I, p. 99.

III

Raven I. MacDavid³⁷⁾ has rightly stressed the many loans from African idioms which also outside the Gullah region have found their way into the American vernacular, thus strengthening the starting point adopted by Mr. Turner. But this also serves to stress the increasing need for a detailed description based on the methods of dialect geography. In this respect it is to be hoped that Mr. Turner's interesting study will form the beginning of a series of studies which may lead to the compilation of a comparative atlas of negro-creole languages and their influence on the lexical inventory of "modern" American.

Finally, a few remarks.

- p. 200: *sakula*, "to eat", etc. is derived from the Bantu noun which is a verbal derivative; cf. Swahili *chakula*, *makuria* (Kimbundu), *kyà kudyà* (Kiluba), *kwesokudla* (Zulu), *bilé* (Lingala), etc.
- p. 192: *do*, "child", according to Turner derived from Mende *ndo*, without, however, referring to the possibility of a derivation from the Bantu stem **-to* with a possible assimilation of the voiced to the voiceless dental in the originally reduplicated form³⁸⁾.
- p. 111: *kinsa*, „from Kongo *kinsa*, Christianity" is not quite correct. Kongo *kinsa*, synonymous with *kinza*, merely means "to worship, to adore" (K. E. Laman, l.c., p. 277); only by adding *kyà Kristu* it obtains the specific sense of "Christianity"; the verb in question may also take *Nzambi* as its object.

The addition to the texts of a number of native proverbs and riddles which usually contain so much traditional material, both linguistical and anthropological, would have enhanced the value of chapter IX.

Perhaps it may seem that in the present review too much attention has been paid to anthroponymy. However, on the one hand several reviewers have already sufficiently stressed the linguistic aspects of

³⁷⁾ Language XXVI² (1950), pp. 328 seq.

³⁸⁾ See W. Bourquin, *Neue Urbantu Wortstämme* (Beihefte zur Zeitschr. f. Eingeb. Sprachen V, Berlin, 1923), pp. 156 seq.; H. P. Blok, *A Swahili Anthology I* (Leiden, 1948), p. 159 note 3.

Mr. Turner's book ³⁹⁾, and on the other the author himself has devoted the major part of his work to an enumeration of personal names of African origin. From the foregoing pages it will have become sufficiently clear that nearly the whole of the theophorous nomenclature, which after all in case of emigration belongs to the most conservative linguistic phenomena, may be retraced to the Western and Southern Sudân. The very concise analysis of the author's anthroponymous material makes a complete and thorough study of these very data highly desirable. For the French language, for instance, Paul Lebel has stressed the socio-anthropological importance of the personal names ⁴⁰⁾.

It is in this respect that Mr. Turner's study seems to fall short; the few pages he devotes in this connection to Gullah personal names (pp. 40—43) in no way provide an analysis, neither from a social and religious, nor from an historical point of view. In view of the data for a so highly isolated group as the Gullah, a comparison with more general descriptions of acculturation etc. of the Negro population in the New World might have been very desirable for America ⁴¹⁾.

The above remarks, inspired as they are by my great interest in the

³⁹⁾ See above note 1.

⁴⁰⁾ P. Lebel, *Les noms de personnes* (ed. PUF, 1949) and the classic on this subject by M. Dauzat, *Les noms de personnes, origine et évolution* (Paris, 1924), as well as the international review published under his auspices, *Onomastica* (since 1947); see also notes 17 and 18 above.

⁴¹⁾ Of the rich literature on the subject, the following works deserve special mention: A. Ramos, *Die Negerkulturen in der Neuen Welt* (Zürich, 1949); W. Zelinsky, *The historical Geography of the Negro Population in Latin America* (*Journ. Negro History*, Washington XXXIV, 1949, pp. 153 seq.); L. L. E. Rens, *The historical and social Background of Surinam's Negro-English* (Amsterdam, 1953). However, all these studies appeared after Mr. Turner's book had already been published; M. J. Herskovits, *The Myth of the Negro Past* (New York, 1941), Chapt. II, and for the earlier history in the mother-country, recently: C. Ch. Reindorf, *History of The Gold Coast and Ashanti based on Traditions and historical Facts* (Basel, 1951) and M. J. Herskovits, *Dahomey. An ancient Westafrican Kingdom* (New York, 1948). For other sources see the preceding notes. The recent book by R. Smith, *The Negro Family in British Guiana. Family structure and social status in the villages* (London, 1956) provides an important contribution to our knowledge of the factors which also throw light on the process of acculturation of a sociological "creolization" in certain mixed communities of negro immigrants and an autochthonous population.

subject, in no way detract from the fact that Mr. Turner's book, because of its conscientious and comprehensive method, may serve as an example to those who will try after him to determine the "African" elements in the present Negro idioms in America on the same scholarly bases. As such, this description of Gullah already deserves full appreciation including the attention of the student of African languages.

Leiden

H. P. BLOK

REVIEWS — COMPTES-RENDUS

B. LEKENS and G. MENS, *Ngbandi Idioticon I, Nederlands-Frans en Ngbandi* (Annalen van het Kon. Museum van Belgisch-Kongo III, no. 1). Tervuren, 1955. Pp. xxvii & 1091.

Ngbandi is spoken in the Belgian Congo by more than 100.000 persons, and one of its dialects, Sango, is the official *lingua franca* for the main part of the area. Although some time ago Dr. TUCKER – albeit with considerable restrictions – believed that the linguistic groups of Sere, Mba, Zande and Banda-Ngbandi had to be included in the “nebulous Sudanic family”¹⁾, it has since then become apparent that numerous Ngbandi word-stems could be traced to basic Bantu etyma. Neither the term “Proto-Bantu”, nor that of “Post-Bantu” should be applied here. Quite apart from the diachronic view inherent in this kind of terminology, which therefore includes a speculative element, it is also impossible to speak about more or less “perfect” types of language. At most one may describe the Bantuid languages spoken in the linguistic borderland between Bantu and Sudanic as being, from a structural point of view, more mobile than Bantu which has become slightly more rigid in its class-system²⁾. The linguistic transition from Bantu to Sudanic, via the still highly problematical Bantuid type of language, is very gradual; it will have to be investigated for every separate idiom and, if possible, it will have to be established according to structural norms.

¹⁾ See A. N. TUCKER and M. A. BRYAN, *The Non-Batu Languages of N.-E. Africa* (Handbook of the African Languages (Oxford University Press, IAI. 1956), III, p. 144 sq. In his *Studies in African linguistic classification* (New Haven, Conn., 1955), p. 12; J. H. GREENBERG includes Ngbandi in the eastern branch of the Niger-Congo family, a term which is more geographical than linguistic. See also D. WESTERMANN in *Völkerkunde von Afrika* (Essen, 1940), p. 385 and the definition “Jeune-Soudanais de l’ouest” by G. VAN BULEK, *Les recherches linguistiques au Congo Belge* (Mém. Inst. Royal Colon. Belge, XVI. Bruxelles, 1948), p. 201, and A. BURSENS, *Inleiding tot de studie van de Kongolese Bantoetalen* (Kongo-Overzee Bibl. VIII. Antwerpen, 1954), p. 12 sq. To the bibliography provided by the author we may add B. THANGE, “Les langues Zande et Ngbandi”, in *Congo II* (1926), p. 203 sq.

²⁾ Conf. J. A. F. WILS, *Het Bantwïde Taaltpe* (Kongo-Overzee XVIII, 4, 1952, pp. 273 seq.

In this respect, the dictionary announced in these lines offers a highly important contribution to the Bantuid material at our disposal which will eventually allow us to arrive at a more specific and a more sharply defined description, as well as at a more certain localisation within the classification of the African linguistic families.

Sango, which is already better known and which is the official *lingua franca*¹⁾ in this part of northern Belgian Congo, is only a dialect of Ngbandi, which is distinguished from this idiom for internal communication and trade as being "the chief's language" or the "aristocratic language". The natives themselves realise the difficulty of this general Ngbandi: "*Yá ngà tí Ngbándi kpwéngbani*" (litt.: "the belly of the mouth of Ngbandi is strong"; p. 751). Beside the most important area around Abumombazi South of the Uele River, two smaller Ngbandi enclaves are to be found enclosed in the Mbanza territory, but these three regions all border to the south directly on Congolese Bantu. In spite of this rather wide geographical diffusion, Ngbandi and its dialects have had a noticeable influence on the surrounding idioms.

The work under review is mainly based on the central data of the Abumombazi idiom. The authors divide their *Idioticon* into three parts: 1. a concise grammatical survey (pp. ix-xxvii); 2. the dictionary proper (pp. 1-1016), and 3. a large collection of proverbs (pp. 1017-1091), arranged according to the Dutch terms and ideas covering the contents and the purport of the locutions quoted. It goes without saying that in an *Idioticon* like the present volume, where each semantic entry is followed by the greatest possible number of expressions and technical terms of a social, religious and anthropological nature, it is this third part which contains a treasure, both of linguistic and of folkloristic data.

From the Grammatical Introduction, several points deserve special mention. The quantitative accent is not distinctive. For the seven vowel phonemes the difference between "long" and "short" is not distinctive either, i.e. it is phonologically irrelevant, an occasional lengthening of the vowels being restricted to the prosody. Like many languages of the Sudan and of Northern Bantu²⁾, the phonemic

¹⁾ In Ubangi, Sango as a *lingua franca* is being increasingly supplanted by Lingála, cf. *Kongo-Overzee* XXI/2 (1955), p. 181 sq.; *ibid.* XXII/4 (1956), p. 407 sq.; *Lingua* IV (1955), p. 333 sq.; A. BURSENS, *op. cit.*, p. 27 sq.

²⁾ For instance in Lingála; see L. B. DE BOECK, *Manuel de Lingála* (Ed.

system possesses the implosive bilabials *gb*, *kpm*, *kpw*, and the velar implosive *ngb*. The tonology of Ngbandi possesses three main tones; combination may give rise to the secondary middle-high and middle-low tonality, whilst the elision of syllables leads either to coalescence or to a shift in the tonality. All verbs have a fundamental tone of their own, to be found in the imperative singular, i.e. a stem-tone ¹⁾, which changes, however, in the derived forms (usually formed by means of auxiliary verbs) ²⁾. The Ngandi verb has three aspects: perfective (the completely finished past), imperfective (the uncompleted action in the present and the future) and consuetudinal, each aspect occurring in the moods indicative, subjunctive and conditional. Also the imperative (with the stem-tone) and the infinitive possess a consuetudinal aspect of their own. The uncompleted action is distinguished from the "future" which belongs to the same aspect, by the lack of a tonality of its own; when the latter occurs, this tends to approach the tone of the perfective aspect. In close agreement with the foregoing is the formation of those consuetudinalia which, both in the perfective and in the imperfective aspect, insert the prefix */ndò* ("indetermined past") immediately before the stem of the verb. However, the sharper temporal distinctions of the more general aspects is attained, like it is often done in these groups of languages, by means of further adverbial determinations.

The noun-system of Ngbandi has neither gender nor case, the former being either expressed by means of several nouns or by circumlocution. The normal possessive (rectum) is usually introduced by *té*, "hand", the specific possessive by *tí* (likewise "hand"). When the relation between

Scheut, Bruxelles, 1952), p. 7 Cf. H. P. BLOK in *Lingua* IV/3 (1955), p. 334 and the literature quoted there.

¹⁾ The four verbal tone-classes of the verbs possess resp. a low, a middle, a high, or a high-low tonality.

²⁾ See the table of conjugations with indication of the tones on p. xxiii sq. In stead of the term "aspect" used in the above, the authors use "tense", considering the consuetudinal as a special case of the "present tense". Compare the native terminology for indicating that a word may change in meaning and function depending on the tone (*légó*): *téne me koi ndógbìnyā ndéndé ndé aléngbì na sè légō me ándodí-ni* (litt.: spraak die een verandert ander-ander-ander is gelijk aan zoals stem waarmede zij het noemen", translation according to the author, p. 773). In view of the printing I have slightly simplified the transcription.

regens and rectum is very close or when the regens has a special meaning (e.g. in names of relationship, place-names, etc.) the noun occurring in the adverbial function may be dropped, but this may also lead to a rise in the tone of the regens. Like in Ewè and in other Sudân languages the verb *hě*, "to give", functions as an indicator of the indirect object.

Usually, the distinction between singular and plural is not indicated by morphological means. Whenever it does happen, the noun concerned is preceded by the high-toned prefix *á*, which forms one "word" together with the noun. In case the noun indicates a person, the plural is described by means of the preceding determinative *áwà*, "people", but as a regens the latter term precedes its rectum without any use of the above-mentioned nouns which function adverbially to determine the possessor. Although as a rule the adjective which precedes the noun it determines is not subject to change, it may in the plural also assume the prefix *á* like the noun.

This brief survey of a few grammatical points is necessary for the user of this *Idioticon*. The lemmata have sometimes been provided with the stem-tone (e.g. in the case of verbs); they constitute the actual dictionary, i.e. the inventory of the fundamental semantemes. The very extensive list of examples and *idiotica*, together with the above-mentioned rich collection of proverbs which concludes the work, contain the practical application of all syntactical and grammatical peculiarities which have been discussed in the preceding lines.

As I said before, the *Idioticon* contains numerous examples which clearly illustrate religious concepts (like e.g. *Nzapà*, *kókòró*, *yòró*, etc.), social-anthropological information about the native way of life (like e.g. *gěnvěně*, *kwá*, and the extensive article concerning the chief and his status in the community on p. 327, s.v. *gbía*), etc.

Ngbandi possesses a large number of "adverbs of manner which reproduce the finest shades of certain actions" (p. xxv), that is to say ideophones¹⁾ which always occur in reduplication. In contrast with the true onomatopoeia ("sound-images") the ideophones are essentially

¹⁾ "A word, often onomatopoeic, which describes a predicate in respect to manner, colour, sound or action ... a distinctive feature of Bantu languages, as also of certain of the Sudanic family" (CL. M. DOKE, *Text-book of Zulu Grammar*, London, 1945, § 625 sq.; the same, *Bantu Linguistic Terminology*, London, 1935, p. 118). Cf. *Bantu Studies* XII (1938), p. 243, and J. MAROUZEAU, *Lexique de terminologie linguistique* (Paris, 1951), p. 114.

“gesture-images”; they are not subject to the prosody of the sentence or to syntax, but they are rather units which possess the nature of interjections and which are practically independent from a grammatical point of view ¹⁾. In Zulu, for instance, where penultimate stress and vowel-length are characteristic features, the ideophones on the contrary always have an initial accent whilst the duration of the vowels is exclusively short. In Lamba there is a fundamental difference in the treatment of the ideophones as regards the normal tone-system ²⁾, etc. In the preceding lines the ideophones were characterized as “units ... practically independent from a grammatical point of view”. However, because e.g. in Ngbandi also in the ideophones a change in the tonality may distinguish the singular and the plural, in so far as these ideophones are felt to be nominal determinatives, we are not entitled to attribute an absolute nature to this functional independence.

Summing up ³⁾, we may observe that Ngbandi, though not a Bantu-language, is rather more orientated towards the South than towards the purely Sudanic language families, both as regards some of its word-stems and as regards other characteristics. Quite correctly Dr. A. N. TUCKER has ranged it together with the Sere and Mba groups under one heading (see first footnote), and thus so the much discussed term “Bantuid” remains for the time being the least debatable classification, for lack of anything better.

The above discussion has amply shown that the book under review constitutes a highly important contribution, not only to comparative linguistics in general, but also to a more penetrating study of the cluster of languages which constitute the transition and the connecting link between the purely Sudanic language-area and the domain of that closely circumscribed unit which is Bantu.

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¹⁾ See the detailed description of the Shangana-Tsonga ideophones and their tones by A. A. JAKES in *Bantu Studies* XV (1941), p. 205 sq.

²⁾ CL. M. DOKE, “A study in Lamba phonetics” in *Bantu Studies* III, p. 38 sq.; the same, *Text-book of Lamba grammar* (Witwatersrand Univ. Press, 1938), § 553; G. FORTUNE, *An analytical grammar of Shona* (London, 1955), § 930, etc. – The numerous ideophones which have been creolised in Georgia and South Carolina and which occur always in reduplication, have been derived in the great majority of cases from West-Sudanic mother-idioms; see L. D. TURNER, *Africanisms in the Gullah dialect* (Univ. Press, Chicago, 1949), p. 236 sq.

³⁾ Conf. MARK H. WATKINS in *Language* XXXII. 3. 1956, pp. 577 sq.

Readings in Linguistics, The Development of Descriptive Linguistics in America since 1925. Edited for the Committee on the Language Program, by MARTIN JOOS, Washington 1957.

It is not generally necessary, in our opinion, to devote a review to the re-issue of a number of articles which have appeared in the past years. There is, however, reason to do so in this case, not so much because it concerns the reprinting of a series of contributions to modern linguistics, some of which are very important, but particularly on account of the connecting texts which the editor has thought necessary to add to these articles.

To publish anew articles of lasting interest which are no longer easily accessible, is in itself a particularly happy thought. Everyone who is unable to consult *Academia Sinica* will be delighted at last to be able to acquire the famous, often quoted article by Yuan Ren Chao about *The non-uniqueness of phonemic solutions*. The same goes for Sapir's *Sound Patterns* in the first volume of *Language*, for Twaddell's criticism (1935) of the definitions of phonemes in use at that time, and for Bloch's article on phonemic overlapping in "*American Speech*", a periodical which can be consulted only in a few places on the continent of Europe. The remaining 40 articles are, however, easily accessible; 30 of them have been included in more or less recent volumes of *Language*, a periodical which is usually available in every university library and to which many linguists outside America, have personal subscriptions. If it was the editor's intention by his *Readings* to give the reader an idea of the development of descriptive linguistics in the U.S. – and the subtitle clearly shows that this was his primary aim –, we must assert that his choice has turned out to be particularly onesided and thereby gives a picture of the development of American linguistics as a whole which is not in the least accurate. This is all the more curious because Joos can certainly not be accused of underestimating what has been achieved in American linguistics in the last 30 years; one has only to read the introduction, in particular page 7. It is at once striking and highly regrettable that Joos apparently did not think it necessary to allot space to the work by Pike, nor to that by Roman Jakobson in a work which claims to outline the recent development of American linguistics, and that also articles by Martinet, Emeneau, Malkiel and Garvin were not deemed worthy of inclusion. It is also to be regretted that, for instance, Haugen and Twaddell's

article in *Language* 18 (1942), and that by Haugen on *Linguistic Borrowing* (*Language* 26; 1950) are omitted, and that the compiler does include his own article in the *JASA* of 1950, (*Description in Language Design*) but not that by Lotz which appeared in the same number of *JASA*, which forms such an interesting contrast with Joos' article. It is also a pity that he did not think Lotz' article in the *Travaux du Cercle linguistique de Copenhague* deserving of a place, nor Trager's article in *Acta Linguistica* on the distribution of phonemes in Polish, and last but not least that the periodical *Word* is only represented by two articles. The variety of opinion and the diversity of interest are certainly important aspects of the present flourishing of American linguistics. This collection does not present a picture of the development, but only of a development albeit – as we are fully aware – an important one.

The connecting text by Joos himself is even more striking than the choice of the articles. We regret to have to remark that it reflects a dogmatic self-sufficiency which is difficult to harmonise with a scientific attitude of mind. To begin with, not only does it perhaps seem somewhat unfortunate and inelegant that the editor has considered it necessary to include articles by himself, but he furthermore makes it clear in various places how important a part he has played in the development of what is, in his opinion, the only truly scientific approach in the last thirty years. What are we to make, for instance, of the following passage: "Altogether, then, there is ample reason why both Americans and (for example) Europeans are likely on each side to consider the other side both irresponsible and arrogant. We may request the Europeans to try to regard the American style as a tradition *comme un autre*; but the Americans can't be expected to reciprocate: they are having too much fun to be bothered, and few of them are aware that either side has a tradition" (preface vii, 2nd column). Apart from various other comments which could be made in connection with this and other passages, it should be remarked that the contrast which is made here between American and European linguistics, is misleading in two senses. Not only is there more in America than the so-called compartmentalization group, to use Pike's term, but it is also inconceivable that one can talk about European linguistics if it is already dubious whether one may accept the existence of a Copenhagen school. The viewpoints of linguists in England, Geneva and

Copenhagen – not to mention other centres – are mutually too divergent (while, on the other hand, being often in accord with views that are defended by American linguists), that it does not make sense to unite them under one label. Joos binary opposition American/European indicates a lack of awareness of the situation of linguistics within and outside the U.S.

Further, in his comment on the article by Bloch on phonemic overlapping, Joos seems to us to demonstrate a too optimistic and dogmatic attitude. Joos naturally honours the hermetic partitioning of levels, i.e. the view that “phonemics must be kept unmixed from all that lies on the opposite side of it from phonetics” (p. 96). He is quite entitled to such a view, but why did he have to add the scarcely gracious reference to Pike’s important articles in *Word*: “the ghost of the slain dragon continued to plague the community of linguists under such names as “grammatical prerequisites to phonemic analysis” and has not been completely exorcized to this day”. Joos would have done better to reflect on the question why this exorcism has not been completed to this day. He might discover that the problem he posed here is far from solved and that, in spite of what has already been written about it, it still demands our full attention.

It is noticeable that, whereas all the articles receive a shorter or longer comment by Joos, the thoughtful article by Haugen which warns against provincialism in American linguistics, has remained without comment. In this case we think a comment particularly desirable, for if Joos had read this article by his Wisconsin colleague thoroughly, it would probably have prompted him to a more sober commentary, and in particular to view recent developments in the history of linguistics in a wider perspective. Finally perhaps, he might have replaced some ten articles or so, and certainly the last four (in themselves no doubt not without merit) by others, for instance those by the authors cited above, which are rightly considered by linguists, both within and outside the U.S., as being far more important.

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J. L. PIERSON, *Key to Classical Japanese: A List of Inflected and Un-inflected Suffixes and Particles of the 7th and 8th Century*. Leiden, E. J. Brill, 1956. Pp. 49. Price: Dutch florins 20.—.

Professor Pierson's *Key to Classical Japanese* is a 'revision and extension' of the grammatical appendix to his *Dictionary of 10,000 Chinese-Japanese Characters* published in 1926. The contents consist of an index of particles and suffixes, a table of 'primary inflections' of the verb and adjective, lists of the particles classified in the traditional manner (i.e., according to the forms to which they are affixed and whether or not they are inflected), and a table explaining the horary and calendric signs. The illustrative examples, given in Pierson's own romanization, are drawn from the Kojiki, the Nihongi, the Kokinshû, the Taketori Monogatari, the Makura no Sôshi, the Genji Monogatari, the Tosa Nikki, and naturally, the Manyôshû. The whole work has that well-honed, comfortable, highly personal look of a tool fabricated for the individual use of its maker, who, no mean craftsman, in this case has demonstrated its adequacy in his monumental undertaking to translate the Manyôshû. No doubt the *Key to Classical Japanese* will be heartily welcomed by all those pupils of Professor Pierson who have been accustomed to use these tables in their original form in the appendix to his dictionary — others, however, are unlikely to take much notice of this book, in spite of its merits, for the simple reason that works of this sort are by no means in short supply. The *Key's* special orientation toward the written language of the 7th and 8th centuries appears only in the peculiarities of the transcription and in the illustrative examples; otherwise it is just a conventional list of suffixes and particles of classical written Japanese and should be judged as such. It makes no pretence of approaching its subject from a scientific linguistic point of view and adheres completely to the traditional presentation of lists such as are found in the appendices of most Japanese dictionaries. It is, therefore, without claim to any special originality. As a simple list with examples it is, moreover, too slight a work to be compared seriously with the grammatical handbooks of Chamberlain, Sansom, Henderson, and others. Finally, the price of twenty guilders for a book of 49 pages (counting in two blank pages, a flyleaf with the title, the title page, and a full page for the copyright notice) can only be regarded as a peculiar kind of joke — a

joke which is unlikely to be appreciated by any one who is aware that similar Japanese lists (much used by secondary school pupils and to be purchased at every bookstall) can be had for as little as seventy-five Dutch cents.

Professor Pierson can not, of course, be blamed for these facts; however, it would seem that his great experience as a translator from 7th and 8th century Japanese and his special insights and intuitions as a Western European would have been much better employed had he, rather than simply duplicating the work of traditional Japanese grammarians, chosen to treat Classical Japanese with the methods of modern Western linguistics. It would be unfair, of course, to forget that the *Key* dates from 1926 and has no doubt been reissued to satisfy the demands of those who became habituated in its use in those days before the war, when many people studying Classical Japanese lacked the ability to use ordinary materials written in Modern Japanese.

Having been obliged to make this realistic statement of the circumstances which destroy the importance of this otherwise highly meritorious work, it is painful to have to speak of its material defects. Nevertheless, it has two which we feel obliged to signal. Firstly, the translations of the illustrative examples are often surprisingly free and sometimes involve expansions of the meaning which are not explainable by the limited context presented. For instance, Pierson renders (p. 43 sub the rubric *to mo*) the expression (in his transcription) '*sidume to mo imasu kami kamo, takara to mo, nareru yama kamo*'! as '(In the land of Yamato) the Goddess resides as one, dominating and maintaining peace, and the Mountain has been born, as the sacred treasure of the land'. This translation certainly squeezes a lot out of a simple rhetorical expression without independent verb, which might be given more prudently and more literally as 'Oh, the Goddess, abiding (with us) as our protectress; oh, the Mountain, created as our treasure!' Admittedly, however, literal translation from Japanese is often difficult and usually involves a certain amount of Gordian-knot-cutting in the supplying of forms for the categories which are obligatorily stated in a western language but unexpressed in the Japanese original. Indeed, in many instances, even where the translation seems unfortunate, it is hard to suggest anything better, and in general, perhaps, we must be grateful to Professor Pierson's skill and experience for having found rather free approximations.

The second defect is one for which we cannot so lightly excuse the author — the more so since it did not occur in the original version of the work. This is the system of transcription employed. In the appendix to his *Dictionary*, Pierson renders all the Japanese forms in the practical Hepburn transcription of the conventional reading pronunciation; in the revised *Key*, however, he has seen fit to use a transcription representing his own reconstruction of Ancient Japanese pronunciation. It must be explained here that Ancient Japanese was recorded in Chinese characters, a Japanese word being written sometimes with its semantically equivalent or approximate Chinese character, at other times with one or more Chinese characters used as phonetic signs. The interpretation of this rather capricious and *ad hoc* manner of writing presents very great difficulties; however, all of these ancient Japanese texts from which Pierson draws his examples exist as a part of living Japanese literature in versions in which the reading has been established in the normal Japanese syllabic orthography. These traditional established versions are the only ones likely to be read by the western students using Pierson's *Key*, and it was therefore unnecessary for him to plunge himself into the difficulties of the phonology of Ancient Japanese. The grammar of Ancient Japanese can be mirrored in modern pronunciation, and should Pierson have chosen to base his grammar on the *kana* orthography or on the modern pronunciation, as he at first did, and as M. Yokoyama has done in her dissertation, *The Inflection of 8th Century Japanese* (Suppl. to *Language* XXVI, 3), no one would have reproached him. As it is however he has committed himself to responsibilities which he does not fulfill. There is in the *Key* no explanation or justification for the transcription to be found. One must look to Pierson's dissertation (*The Manyōshū*, Leiden, 1929) for the details of his reconstruction of Ancient Japanese pronunciation. For him to resurrect this reconstruction in a work published a quarter of a century later without a word of apology is most surprising and leads one to suspect on the author's part either a disinterest in or a detachment from the very important developments which have been made in the study of Ancient Japanese phonology in the intervening time. To touch on only one conspicuous point, it is quite clear that Ancient Japanese distinguished a greater number of vowel phonemes than are recognized in the traditional *kana* texts. In the *Manyōshū*, two distinct series of Chinese characters are used to

record each of the syllables rendered in the *kana* as *e, ki, gi, ke, ge, ko, go, so, zo, to, do, no, pi, bi, pe, be, mi, mo, yo, and ro* (cf. S. Hashimoto, *Kodai Kokugo no Onin ni tsuite*, Tôkyô, 1942). The nature of this distinction is of course a matter for surmise—perhaps an opposition of rounded/non-rounded? Whatever it may have been, a transcription for the pronunciation of 7th and 8th century Japanese which suppresses it as Pierson's does can scarcely fail to provoke strong objection.

Oegstgeest

DON GRAHAM STUART

D. J. WIJAYARATNE, *History of the Sinhalese Noun, A morphological Study based on Inscriptions*. (University of Ceylon, 1956), XXV + 217 pp.

Among the many Indo-Aryan languages Sinhalese is particular in several respects. Introduced as a foreign language by Aryan-speaking immigrants some centuries B.C., it was adopted by the aboriginal inhabitants but probably underwent the influence of the non-Aryan substratum. This is indeed held responsible for the vast and rapid changes that Sinhalese went through during the period from the 3rd c.B.C. to the 8th c.A.D. These changes definitively determined the character of the language. In the course of time Pali, Buddhist and Classical Sanskrit have contributed to the development of Sinhalese, whereas the conquest of the northern part of Ceylon by the Tamils from the continent brought the Sinhalese-speakers into close contact with one of the most important Dravidian languages. To this Dravidian influence, which is also noticeable in the syntax of late Pali texts written in Ceylon, several features of Sinhalese have been attributed. Now this language is not only an interesting object of study because of the intricacy of its various linguistic strata, but it is also of particular importance to the study of diachronic linguistics on account of its "remarkable wealth of recorded material available from the 3rd c., B.C., up to the present day, representing every century, . . . a record which few other living languages in the world can claim" (Preface, p. IV).

In spite of this favourable circumstance, however, it is generally realized that for many years to come it will be impossible to write a comprehensive account of the historical development of Sinhalese. Many monographs will have to pave the way for such a synthesis. After the first contribution in this field, Dr. P. B. F. Wijeratne's Ph.D.

thesis on the "Phonology of the Sinhalese Inscriptions up to the End of the Tenth Century, A.D." (BSOAS. XI, 580 ff., 832 ff.), the work under review (also a Ph.D. thesis of the London University) is devoted to a morphological aspect of the language during the same period. It is based on epigraphical material gathered from the inscriptions from the 3rd century B.C. to the 10th century A.D. Since the reviewer cannot claim any expert knowledge in this field he must confine himself to observing that Dr. Wijayaratne's work contains an accurate account of the available evidence, and discusses many problems which are also of interest to the student of Indian linguistics generally, e.g., the loss of the indication of number, the replacement of grammatical gender by "notional gender", the introduction of an indefinite suffix (m. *-ek*, f. *-ak*), etc. The historical explanation of many details presents unsolved difficulties.

As is mostly the case when languages of a different structure come into a close contact, one often gets the impression of a convergent development without being able to account for the details of it. Sinhalese appears, in several points of its grammatical structure, to approach the Dravidian type of language as represented by Tamil. Thus Wijayaratne, while discussing in detail the unsolved problem of the origin of the plural ending *-val* (attested since the end of the 10th century) points out that, as a result of its introduction, the relation between the Singular *gē* "house", Instr. *ge-y-in* and the Plural *ge-val*, Instr. *ge-val-in* has become quite parallel to that of Tamil *maṇai*, Instr. *maṇai-y-āl*, and Plural *maṇai-kal*, Instr. *maṇai-kal-āl*. Now, although the Tam. phoneme /k/ is often pronounced intervocally as [v], especially by lower castes (cf. J. Bloch, MSL. 16, 1910, p. 8), this parallelism does not lend the slightest support to Ed. Müller's suggestion that Sinh. *-val* may have been borrowed from Tamil and be identical with *-kal*. First, the tendency common to all New Indo-Aryan languages to form plural forms by compounds consisting of a noun and a general word denoting "multitude", "troup", etc. must automatically result in Instr. forms of the type *ge-val-in*. Second, whenever the grammatical system of an Indo-Aryan language appears to have been influenced by Dravidian, it *imitates* the Dravidian procedure by means of Aryan morphemes. Therefore, the parallelism between Sinh. *ge-val-in* and Tam. *maṇai-kal-āl* may be merely the result of a convergent development. The convergence in itself, however, *may* become inter-

esting, if several different phenomena betray a similar tendency. Now Sinhalese has indeed some instances suggestive of imitation of a foreign model that seem rather significant. One of them is the development of a separate form to indicate the syntactical function of the Ablative of separation of inanimate nouns. While originally the common Abl. Instr. case-form was employed exclusively, we find since the 10th century beside the old form *gam-en* "from the village" the periphrastic expression *gāmāhi siṭa*, lit. "in the village" (Loc.) "having stood (or, remained)". Although analogous constructions are stated to occur in Apabhramśa and Gujarati, the parallelism with Tam. *ūrīninru*, id. (composed of the Loc. *ūr-il* "in the village" and *ninru* "having stood") is too striking to be explained away as merely accidental. On the other hand, the extension of such normal phrases as *raje mi* "I am king" to the quite peculiar type of expression *Puyagonula-mi* "I, Poyagonula, . . ." (Inflected animate nouns in the first person singular, p. 173) may be explained as the result of a specifically Aryan development, although the result is reminiscent of Tam. *aṭi-y-ēn*, which may mean both "I am a slave" and "I, who am a slave". In such cases, indeed, it is mostly impossible, if a foreign linguistic system has exerted some influence at all, to produce a scientific proof of it.

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F. B. J. KUIPER

Der Pretakalpa des Garuḍa-Purāṇa, Eine Darstellung des hinduistischen Totenkultes und Jenseitglaubens. Aus dem Sanskrit übersetzt und erklärt von EMIL ABEGG. Zweite, unveränderte Auflage. Berlin, Walter de Gruyter & Co. 1956.

This is an (apparently photo-mechanical) reproduction of Abegg's well-known book, the first edition of which dates from 1921. While Caland's and Raabe's studies had made known the details of the Vedic funeral rites, little was known at that time about the cult of the Dead in later Hinduism. The main part of Abegg's book is a translation of the relevant chapters of the Garuḍa-Purāṇa, which is followed by two indices. As it is still the most important work in this field, this re-edition will be welcomed by all those interested in this aspect of Hindu culture.

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JOSE PEDRO RONA, *El Culto indoeuropeo del Fuego*. Universidad de la Republica, Facultad de Humanidades y Ciencias, Instituto de Filología, Departamento de Lingüística, Montevideo 1957, 26 pp.

That the publication of this study (which appeared simultaneously in the *Boletín de Filología*, vol. 8, Nrs. 49-51) was a mistake is too obvious to need a circumstantial demonstration. The linguistic argument looks like being written in the years before Jacob Grimm. Suffice it to mention that not only Greek πῦρ and Sanskrit *pure* "fire, sun" (?? Umbrian?), but also Old Engl. *bearn* (sic) "to burn" are referred to an IE. root *ph₁/per*, and that Greek φέγω and Latin *fugo* (sic) are connected with Slavonic *běgati*. So the sole thing one could do is advising the author to postpone the publication of his greater work which he promises us (p. 25, note 1) until he has acquired the indispensable basic knowledge and methodical training required for studies of this kind, . . . if this advice were not to reach him too late. Since Rona is keenly interested in his subject, it is a pity that he should not even have such authorities as Walde-Pokorny and Pokorny at his disposal (I wonder, what may have been the source of such "Sanskrit" words as *pure* and *paci* for "fire"). But even more reliable works than those of Bartoli and Hrozný, in which he appears to have an excessive faith, would not make up for his lack of methodical training. It is sincerely hoped that Rona may find an opportunity of studying for some time in one of the centres of Indo-European linguistics.

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